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ANDRÉ MARTINET AND URIEL WEINREICH

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WORD

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THE UNITY OF LINGUISTICS

ANDRÉ MARTINET

The unity of linguistics! A reality or a pious hope? To many of those for whom it is a goal, this goal seems more remote than ever, with large conservative groups blind and deaf to whatever did not receive public sanction from long defunct leaders, with "progressive" schools, factions, and individuals trying to assert themselves at the expense of their rivals, attacking these, ignoring those, wooing potential followers, looking for support and allies in more or less contiguous fields, playing now the card of national protection and next time that of intellectual free-trade, all according to the changing needs of their policies. Is there, in any other domain of research, such a seemingly unbridgeable gap between the ranks of disciplined, painstaking, often gifted, but pusillanimous and unimaginative, tradition-bound scholars, and the free-for-all of modern "structuralism" with its swelling crowds of impatient and sketchily informed would-be theoreticians? It is true that, both in Europe and America, work is in progress toward restoring the unity of linguistics: Here and there new generations of scholars are entering the field, so equipped that they can apply new theories to traditional subjects and avoid empty theorizing. But this has just begun, and on a limited scale. Disunion is still rife, and its varying forms deserve to be surveyed as so many features of today's linguistics.

First of all, do people agree as to the meaning or at least the implications of the term "linguistics"? Nothing is more conducive to international misunderstandings than deceptive cognates, "faux-amis" as the French more picturesquely call them. The thing is too well known to need illustration. But, forewarned and forearmed as we are, we may still be tripped when formal equivalence combines with enough semantic agreement to permit one and the same definition, and yet implications of all sorts, factual or emotional, vary from one language to another. The term 'linguistics' is a case in point. The word, in its various national garbs, would be defined everywhere as 'the scientific study of language' and thereby seem semantically to tally with such a formally divergent designation as *Sprachwissenschaft*. Yet, its content is, from one language or better perhaps, one usage to another, variously colored by the specific conditions under which the study of language came to assume some sort of rigorous character and to win some measure of academic recognition. Particularly in English speaking countries, where what in other quarters had been going under the names of 'linguistique' and 'Sprachwissenschaft' was traditionally known as 'philology', the word 'linguistics' tends to imply the existence of a new order of research with few or no ties with the past.

In those quarters where we notice a certain disregard of former research and previously established terminological habits—a practice highly detrimental to international cooperation—it may, to some extent at least, derive from the belief that 'linguistics', a new term, designates a novel approach to the study of one aspect of man's behavior. Wherever a local designation coexists with 'linguistics' (such as *Sprachwissenschaft* by the side of *Linguistik*), scholars will, consciously or unconsciously, be tempted to differentiate between the two by specializing the latter according to their personal experience, irrespective of course of international usage. At best there will be an agreement in principle on the meaning of the term 'linguistics', but this will not prevent every scholar from entertaining the view that linguistics properly so-called is just that aspect of the science of language with which he is particularly concerned.

All this is hardly new, and our predecessors were even more biased than we are: not more than thirty years ago, there could be published, under the most unspecific title *Stand und Aufgaben der Sprachwissenschaft*, a 670-page volume devoted to Indo-European comparative linguistics. But since, in those days, most linguists of repute shared the same bias, this could not be detrimental to the unity of the discipline. It seemed obvious then that linguistics amounted to a comparison of genetically related languages with a view to establishing correspondences between them, and that such a comparison would be the more valuable if it could be made along the time axis. This of course put early attested language families, the Indo-European one in particular, in a favored position, but one which few responsible scholars dared challenge.

On the whole, the unity of linguistics was never questioned or jeopardized as long as it was essentially a German undertaking, with only a handful of scholars in Italy, France, America, and elsewhere, at times opposing and, half a century ago, successfully attacking German supremacy, but conforming all the time to the German rules of the game. How difficult it is to maintain international cooperation on a democratic basis is well illustrated by the stand currently taken by Indo-Europeanists concerning the so-called laryngeals. Generally this stand is national rather than individual: the Germans and Italians are, with rare exceptions, definitely hostile; the French, Spaniards, and Norwegians are tempted but wary; the Danes favor as few of these as possible; the rank and file of American comparatists operate with four, phonetically perfectly pinned down, laryngeals. It is true that many features of Brugmann's reconstruction were never universally accepted as facts, but no one concerned refused to use them as terms of reference, and this was enough to secure mutual understanding from one school or one country to another. As long as scholars revered Saussure as a great mind, but kept the most explosive of his ideas carefully stored away, linguistic research could proceed along traditional lines at a sufficiently slow pace for any innovation to be digested or eliminated before the next one appeared. Meillet was the only one who managed to make common property of important aspects of the Genevan's teachings, stopping with consummate skill at the very point where one more daring move would have made the conservatives wince and balk.

Both in general linguistics and in the Indo-European comparative domain, it is the insistence of some linguists on drawing all the consequences from the most revolutionary among Saussure's ideas, that marked the beginning of a new era in linguistic studies. A point was reached when the urge to go forward got the better of the time-honored custom not to sever contacts with one's fellow-scholars. Until then, responsible linguists had not taken a new step forward without having ascertained that the preceding step had met with the agreement of some of their peers, the well-considered and carefully documented rejection of the others. Now that all this changed, rifts were sure to follow and did follow. Had the revolutionists and the traditionalists been engaged in pretty much the same pursuits, contacts would have been unavoidable; results achieved on one front would finally have attracted the attention of those toiling on the other front. But since the former were intent upon developing methods of synchronic description, whereas the latter went on considering every fact along the time axis, the two groups, after a few initial clashes, could proceed along their respective lines, comfortably ignoring each other.

Today contacts are becoming more and more frequent, and yet it is probably no exaggeration to say that the average member of one group is genuinely incapable of attaching any meaning to the activities of the other group. A "historicist" will be just blind to the bundles of intimate connections which the synchronist points out between the different units of a language system; "descriptivists" of the younger generation may be so exclusively engrossed in these connections that they fail to see the point in determining that, e.g., English *o* in *stone* is the normal outcome of an Old English *ā*.

The resuming of contacts may have been appreciably delayed by the fact that the compact troops of the "descriptivists" are separated by an ocean from the mass of "historicists": whereas the latter retain the upper hand in most of Europe, they have, in America, been definitely shoved to the background. The rapid progress of "descriptive" linguistics in the United States can easily be understood when one remembers that, under the leadership of Franz Boas and Edward Sapir, generations of young American scholars had been engaged in recording Indian languages. Some amount of comparison, aiming at establishing genetic relationship, was bound to follow. But, in those fields, time-depth was not documented and had to be reconstructed. And when one remembers how little of it former Indo-Europeanists managed to introduce into their reconstruction in spite of most favorable circumstances, Americanists may be excused for concentrating less upon time perspective than on the more pressing task of linguistic description.

Since man, as a rule, takes language for granted, interest in language can only be aroused when there is something the matter with the unimpeded use of it. This explains why those who took care of deaf-mutes were, in many places, the first ones to give accurate linguistic information about the vernaculars. An incentive will also exist when people seek a better understanding of old texts whose message is felt to be culturally vital. Hence philology and that type of comparative linguistics which ultimately derives from the study of ancient documents.

Originally, American Indian languages were not studied, by Boas and his disciples, for their own sake, but in order to establish more direct contacts with other types of culture. But the right understanding of the cultures presupposed a correct understanding of the languages, and this could not be achieved without a painstaking and protracted study. This study proved intellectually so rewarding that, what was first meant as a means soon became an end, at least with some scholars. Yet, by anthropologists in general, linguistics would be conceived as an auxiliary discipline, best practiced by scholars with a good general anthropological background. Anthropological pursuits would at times yield results likely to catch the fancy of a wide and wealthy public, and it was economically sound for American linguists to maintain their, not unnatural, ties with anthropology. As a consequence they are apt to make use of the same jargon, with words like 'overt', 'covert', 'acculturation' used for concepts with which more traditional linguistics had long since been operating under different labels. It is not implied here that the old terms were necessarily better, and it is of course highly desirable that all the scholars concerned with the study of a given culture should be given a chance to understand one another. But it is also desirable that no terminological barriers should be raised between the various categories of linguistic specialists. Both ends, in many instances, could be secured by using non-technical everyday language: would we, for instance, miss much of what is intended if anthropologically trained linguists spoke of "learning a new language" instead of "linguistic acculturation"?

The abuse of jargon, the one among the awe-inspiring techniques whose practice most conveniently combines with least effort, is of course not a specialty of anthropological linguistics, nor, for that matter, of linguistics in general. It is clear that any advance in any field of knowledge will entail terminological innovations. According to the very laws of linguistic evolution, a concept, which could stand being expressed through a phrase when its use was occasional, will, one way or another, secure of handier rendering as soon as its frequency increases beyond a certain point. A technical jargon is an unescapable necessity in every field of research. But unbridled terminological expansion automatically results in making more difficult the cooperation between the various schools of thought. If individuals and audiences were always candid enough to indicate to the speakers the extent to which they have or have not understood them, the latter would more vividly realize how detrimental the abuse of jargon can be to the diffusion of their views, and they would probably impose upon themselves the lexical discipline which often makes all the difference between expressing one's self and conveying one's ideas to others.

The danger is serious enough when linguists are left to themselves. It is multiplied when they decide to cooperate with specialists from other fields. Cooperation between the various orders of research can be extremely fruitful for them all, but it can only be so when a body of interpreters is at hand who can present the discoveries made in one science in the language of another. It is foolish to expect that linguists as a group will find time, energy, and inclination to acquaint themselves with the wide variety of extraneous disciplines from which

some light could reach them. Flaunting mathematical formulae before a linguistic audience or in a linguistic publication is either grossly misinterpreting the needs and capacities of one's audience or readers, or else trying to bully them into accepting one's views by claiming for these the support of a science they tend to respect as the most exact of all sciences, but whose data they are not in a position to verify. We need more and more rigor in linguistics, but our own brand.

The unity of linguistics is to be found in the overcoming of the Saussurian antinomy between diachrony and synchrony. In simpler and more specific terms, it will result from the recognition that a real understanding of what a language is at a certain period, of how and why it changes from one age to another, can only be gained through a careful observation of how it serves the needs of all the sections of the community, the different generations in presence, the various social levels and dialectal subdivisions, and how it may, toward that end, have to compete with other languages. Instead of trying to prove the validity of their own structural theories at the expense of a language, descriptivists will have to order all the facts of that language according to their respective role and importance in its economy. Instead of brushing away as irrelevant, or disregarding as subordinate, semantics, phonetics, or both, they will have to look for information wherever information is to be found, taking of course great care to evaluate the reliability of every piece of it. The description of a language is not achieved through taking apart all the elements of its delicate machinery any more than a watch would be usefully and exhaustively described through the linear display on a green cloth of all its springs and cogwheels. It is necessary to show how all the elements of both the language and the watch cooperate when at work. Anatomy, unless studied with a view to accounting for physiology, would amount to some sort of 'necrology' or corpse-lore of little use or interest to anybody except perhaps professional embalmers. So far we have had, in descriptive linguistics, a little too much anatomy and not enough physiology, and the rigor after which some of us are striving too often resembles rigor mortis. But no analogy is fully satisfactory: the nature of the watch depends on the decisions of the watchmaker infinitely more than on its own functioning, and it would be for biologists to decide to what extent living specimens carry within themselves some of the germs of the future evolution of their species. In the case of languages, observation will show, not only how they function today, but also how the ever changing and conflicting needs of their users are permanently at work silently shaping, out of the language of today, the language of tomorrow.

Columbia University

THE CHOICE OF CRITERIA IN STRUCTURAL LINGUISTICS

C. E. Bazell

Discussions on the question of criteria are often vitiated by the attachment of question-begging labels to the criteria under consideration. Not only such ambiguous terms as *formal*, but even such innocent-looking words as *phonetic*, may be used to draw false oppositions, and hence to give an unfair twist to the argument.

The first illustration of the point which is being made will be taken, however, from an account of modern linguistic tendencies which is throughout conspicuously fair to rival viewpoints, and where the spurious contrast draws prejudices rather than reënforces the general argument of the author. In a recent paper[1] E. Haugen writes:

"Attempts have been made to define sound-classes purely in terms of distribution, e.g. in Hans Vogt's study of the structure of Norwegian monosyllables.[2] The results are interesting, but they show chiefly that phonetic identity and distributional function do not coincide. A definition of vowel versus consonant in terms of syllabic distribution is similar to that derived from a phonetic analysis, but not identical with it; the difference appears when we consider the *n* in *button*, which is a consonant phonetically but functions as a syllabic."

It is evident that the two criteria do not differ through the fact that the first is phonetic while the second is not; the fact that *n* may be syllabic is no less a phonetic fact than the fact that it is a consonant. But the word *identity* does not serve either to distinguish the one criterion from the other. For both criteria are criteria for identifying phonemes. It would surely be very odd to say that there are two criteria for identity, one of which is identity itself, while the other is something else.

Actually the term *phonetic* belongs to neither side of the opposition; since everything (including distribution) is phonetic in the medium concerned, it is superfluous. On the other hand *identity*, or better *similarity*, belongs to both sides of the opposition. The opposition is between *intrinsic similarity* and *distributional similarity*.

In order that *intrinsic* and *distributional* may form an appropriate opposition, it is necessary to take *intrinsic* in rather a wide sense, a sense so wide that, for the purpose of linguistic analysis, distribution remains the only *extrinsic* relation. Hence we shall regard as intrinsic the relation stressed by A. Martinet in contrast with what might, in a narrow sense, be described as "intrinsic identity": "si nous identifions le *b* de *banc* et celui de *bout*, ce n'est pas parce qu'en les comparant l'un a l'autre, nous jugeons qu'ils sont trop analogues pour ne pas représenter la même unité différenciative, ni exactement parce que le *b* de *banc* nous paraît plus près du *b* de *bout* que du *p* de *pou*, mais uniquement et exacte-

[1] E. Haugen, Directions in Modern Linguistics, *Language* 27.211–222 (1951); p. 218 cited.
[2] *Norsk Tidsskrift for Sprogvidenskap* 12.1–29 (1939).

ment parce que nous constatons que le *b* de *banc* se distingue des autres consonnes de la série *banc, pan, van*, etc. par les mêmes traits phoniques qui assurent la distinction entre le *b* de *bout* et le *p* de *pou*, le *v* de *vous*, le *f* de *fou* etc."[3] In other words the decisive criterion is not the intrinsic resemblance of the terms, but the intrinsic resemblance of the *relations* between the terms. But in either case we have to do with *intrinsic* similarity, as opposed to the *extrinsic* similarity of distribution. Similarity of distribution is called extrinsic, not because it is relational (for so also is the similarity stressed as decisive by Martinet), nor yet because it is not phonic (for it *is* phonic), but because it is based on relations holding independently of the shape of the units. (Very strong is the temptation here to say "the *phonetic* shape of the units," as though there were some other shape with which this could be contrasted; and as though such relations as *after*, on which statements of distribution are based, were not equally *phonetic* relations.)

The one-sided use of the term *phonetic* has given rise to the notion that there is something uniquely *formal* about distributional relations. But intrinsic identity is no less formal a matter than distribution, and the distribution of *phones* is no less substantial a matter than the intrinsic identity of *phones*. When we make a formal comparison between different mediums (e.g. spoken and written English) this comparison is based alike on intrinsic identities within each of these mediums and on the distribution of the identified units.

This peculiar notion has also another source. A *formal* comparison between two systems using the same sort of medium (e.g. spoken English and spoken French) is a comparison in which substantial resemblances are disregarded and only the distributional relations of abstract terms are considered. But to say that substantial relations are disregarded is of course to say that *inter-substantial* relations are disregarded. It is not to say that *intra-substantial* relations (the relations between the phonic features in each of the media considered separately) are in any way neglected. If they appear to be neglected, it is simply because they are taken as already known. A formal comparison is a comparison between the *distributions of identities*. Irrelevant are simply the substantial identities or resemblances between the units of *one* system and the units of *another*. There was never any question of resemblances *within* a given medium of a given system being irrelevant.

One cannot therefore follow Miss E. Fischer-Jørgensen when, in her excellent article[4] on phonemic analysis, she takes the glossematic identification of Danish initial *t* with medial *d*, and of initial *d* with medial *ð*, as a sign that the substance has not played a decisive role. Within a given medium (of a given system) the distinction between formal and substantial criteria has no application. Both L. Hjelmslev and R. Jakobson[5] accept the identifications for the same reason, namely that they hold the substantial relation between *t* and *d* to be similar to

[3] A. Martinet, Où en est la phonologie?, *Lingua* 1.34–58 (1949).

[4] E. Fischer-Jørgensen, Remarques sur les principes de l'analyse phonémique, *Recherches structurales*, Copenhagen 1949, p. 225.

[5] R. Jakobson, On the Identification of Phonemic Entities, *Recherches structurales*, p. 211.

the substantial relation between *d* and ð, and the substantial distribution to allow the identification. It is the intrinsic similarity of the relation *t*/*d* to the relation *d*/ð which justifies the choice of this identification rather than others (e.g. the identification of the intrinsically similar *terms* "initial *d*"/"medial *d*"). If on the other hand Martinet hesitates to accept the identification,[6] his grounds are neither more nor less substantial than those of the previous scholars. Nor (but this is a different question) are these grounds in any way connected with the identification of terms rather than of relations; in view of the citation above, such an assumption would be quite paradoxical. The difference lies rather in this, that while all the linguists in question are agreed on the importance of the relational criterion and of its applicability to this case, they are not all agreed on whether the criterion is fully satisfied. One may take the view that two of the scholars have too lax an idea of relational similarity, or one may take the view that the third scholar's demands are too rigorous. One may also take the view that the case is a marginal one, and that no final decision can be reached. But all these views are views about phonetic relations, and the more lenient phonetic attitude is not a whit less substantial than the stricter attitude.

The whole question of formal and substantial criteria, as applied to the identification of units within a given system and a given medium, is a mistaken question, which arises through the assimilation of the distinction *form/substance* to other distinctions, such as the distinction of extrinsic and intrinsic similarity, or the distinction between relation and term, or even (as would appear from the example just cited) the distinction between a lax and a rigorous attitude towards substantial similarities.

From the question of formal and substantial *criteria* must be sharply distinguished the question of formal and substantial *definitions*. Even when only one substance (medium) of one single system is under consideration, this latter question remains significant. Once all the *identifications* have been made, on grounds that can only be substantial, it still remains a question whether the *definition* is best stated in formal terms.

But the answer to this question will depend on the purpose of the definition. If the purpose of the definition is to enable a comparison between spoken and written language, the definition must obviously be formal, since there are no substantial resemblances to record. If on the other hand one is interested in substantial similarities between different spoken languages, the definition will be substantial; and such definitions are by no means uninteresting to the formalist, who may be interested precisely in the question how far substantially similar units, in different languages, have similar *formal* definitions. Such questions cannot be answered without a prior substantial definition.

Unfortunately we are not yet at an end with the distinctions which have falsely been equated with the distinction of form and substance. There are at least two others which must be mentioned. The first of these is the distinction of the functional and the non-functional. It is no doubt partly this confusion that R. S. Wells has discussed in his careful review[7] of *Recherches structurales*,

[6] *La phonologie du mot en danois* (reprinted from *BSL* 38,2, 1937), p. 43.
[7] *Language*, 27.554–570, esp. p. 566.

though he has in mind at the same time the confusion with the distinction of relation and term. Now the distinction of the functional and the non-functional is simply a criterion of what does or does not belong to language in the narrower sense. Allophones may be excluded from the system on this ground. But allophones are neither more nor less capable of formal treatment than are phonemes. If they are excluded as irrelevant, then their form and their substance are *alike* excluded as irrelevant. It would be a mistake to regard allophones as the substance of phonemes. The substantial side of the phoneme is simply the substantial feature common to the different allophones,[8] just as the formal side of the phoneme is the distribution of all these allophones, considered as a single range of distribution.

The other confusion, and the last to be discussed here, is the confusion with the distinction of distribution and composition. Since a minimal unit has a distribution but no composition, the formal definition of a minimal unit is naturally only in terms of distribution. But the classification of non-minimal units, such as the phonemes, in terms of distribution rather than of composition, cannot claim the advantage of being formal. Z. S. Harris' analysis[9] of Swaheli phonemes in terms of distribution is neither more nor less formal than R. Jakobson's analysis[10] of French phonemes in terms of composition. If formal analysis meant analysis in terms of distribution, it would not be sufficient that phonemes should be analyzed into features in this way. We should also have to analyze syllables into phonemes on the basis of the distribution of syllables, and words into syllables on the basis of the distribution of words. Such so-called analyses would not be analyses at all, but simply restatements of distributional facts.

Of the terms used by Jakobson in his feature-analysis,[11] some are formal and some are substantial, without this difference being of any consequence to the analysis. Such terms as *nasal* are frankly substantial, and can apply only in phonetics (at least when taken at their face-value). Such terms as *compact* and *diffuse*, on the other hand, are formal terms, which could equally well be used to describe differences of graphic form. It might be urged that, when the opposition of *compact* and *diffuse* is regarded as being the same opposition in languages having complex units (e.g. compact-diffuse vowels, intermediary between the maximally compact and the maximally diffuse), and in languages which do not have such units, this implies a disregard for form. Actually, it merely implies that this is not the sort of form which happens to be at the center of interest. There are many types of formal definition, and the distributional type is only one. Nor on the other hand are distributional definitions necessarily formal. If by the distributional definition there is explicit reference to the occurrence of

[8] It is possible of course to set up phonemes without claiming that the allophones all have something in common; cf. e.g. C. F. Hockett, Two Fundamental Problems in Phonemics, *Studies in Linguistics* 7.38–39 (1949). But this is simply to say that the phoneme has no substantial equivalent.

[9] *Methods in Structural Linguistics*, Chicago 1951, pp. 136–143.

[10] R. Jakobson and J. Lotz, Notes on the French Phonemic Pattern, *Word* 5.151–8 (1949).

[11] R. Jakobson, C. G. M. Fant and M. Halle, *Preliminaries to Speech Analysis*, Cambridge 1952, pp. 27 and 34.

sounds in *temporal* sequence, the definition is not formal. If the spatial sequence of letters answers equally well to the definition, then this is formal.

It remains true that the sort of form in which linguists are primarily interested is distributional form. No linguist has troubled to point out (even by way of an academic joke) that the formal equivalent of the correlation of gemination is also to be found in the letter-opposition *v/w*. The fact happens to be without any interest. On the other hand Jakobson and others have stressed the interesting formal relation between vowel-systems and color-systems, a relation which has evidently nothing to do with distribution. The importance of distributional relations in linguistics is only too evident; nothing is added to it by bringing in the here quite irrelevant distinction of substance and form.

Hjelmslev has said that substance presupposes form whereas form does not presuppose substance; whereas form is a constant, substance is variable.[12] Doubtless there is a sense in which one may say that a variable presupposes a constant, while a constant does not presuppose variables. If so this is a tautology from which no empirical consequences can be drawn.

Two substantial mediums have the same form when, to every term or relation in the one, there answers uniquely a term or relation, respectively, in the other. In every actual case it has of course to be decided what features of the two mediums are to be regarded as essential. If every feature were to be regarded as essential, isomorphism would be confined to a few specially devised cases, such as oral speech and the new technical discovery "visible speech." In order to regard normal spoken and written languages (even in the ideal case of phonemic transcription) as having the same form, all relations except that of sequence would have to be disregarded, even the relation of simultaneity being excluded through a refusal to treat the composition of phonemes as relevant.

However linguists appear to be in general agreement over the principle to be applied, whether they use the term *commutation*, or *non-redundancy*, or quite simply *relevance* (better *communicative relevance*).

There are of course numerous differences in the application of this principle, but it is recognizably the same principle in all schools of structural linguistics. The differences between the Yale School and most European schools have surely been exaggerated. It is true that the former school insists on distinguishing between commutability and mere substitutability (free variation) by purely distributional criteria, without making a direct appeal to semantics.[13] However the distributional criterion applied, namely the possibility of recognizing free variation by the occurrence of otherwise similar utterances having the same phonic difference, would no doubt everywhere be recognized as a good objective criterion of what is meant by meaninglessness. If two words have different

[12] *Omkring Sprogteoriens Grundlæggelse* (Copenhagen 1943), p. 94. The affirmation that substance is not necessarily presupposed by linguistic form would be disputed by some scholars even with deletion of *linguistic*. Even mathematical logicians are by no means so universally agreed on the purely formal nature of their subject as many linguistic formalists are apt to suggest.

[13] Cf. B. Bloch, A Set of Postulates for Phonemic Analysis, *Language* 24.3–37 (1948); Contrast, *Language* 29.59–61 (1953).

meanings, they will not be found indifferently in the same environments. It has been said that "indifferently" here begs the question, since it is only on the basis of meaning that we know that substitution will not invariably yield a possible utterance. However it might equally well be said that the fact that the meanings are different is only learnt in large part by observation of the different ranges of normal linguistic environment, direct confrontation of utterances with "situations" playing a minor role. As for the other common objection,[14] that the variety of speech-sounds is unlimited: if two utterances could in no instances ever be regarded, at least provisionally, as differing in only one respect, there would be no material to which the criterion of semantic distinction might be applied. There has to be some provisional segmentation, and some provisional identification of one segment with another, before there are any units about which it may be asked whether the meanings are the same. Moreover, if we demand of semanticists a rigor equal to that which is here demanded of the distributionalists, we have to demand that they continue to apply the criterion of relevant substitution to all possible segments, in view of the possibility that a substitution which has proved irrelevant a thousand times might prove relevant the next time. Any theory whatsoever can be made to appear ridiculous by supposing that it will be applied in an absurd manner. But in fact it should be supposed that, just as a large number of irrelevant substitutions will serve as *prima facie* confirmation of free variation (when there are no examples to the contrary), so also will a large number of repetitions of rather similar utterances differing in this or that respect.

The difference between the glossematists and other European schools is a more subtle one. Glossematists say that a distinction in expression is relevant when it answers to a distinction in content, *and vice-versa*.[15] This is the principle of commutation. One may start either from expression *or from content*.

What remains unclear is how it may be discovered that there is a difference of content. The normal answer to this question would be, that if a unit may be indifferently substituted for another, there is no difference in content. But this answer is, of course, simply a repetition of the principle of commutation. In order to tell whether the contents are different, we have first to see whether the substitution of the expressions serves to differentiate communications. But to have done this, is already to have answered the question. There are not *two* questions, namely (i) whether the contents are different, and (ii) whether the different expressions answer to different contents. There is one question only. The criterion of commutation does not operate on two previously given substances, expression and content. The principle of commutation *is* the principle of content.

The notion of a "substance of the content" which, it appears, may be thought of either from the mental standpoint or from the physical standpoint,[16] derives perhaps from the old identification of *meaning* with *naming*. What a word *names*

[14] Cf. e.g. E. Fischer-Jørgensen in *Recherches Structurales* (*loc. cit.*), p. 219.
[15] Cf. L. Hjelmslev, *op. cit.* p. 67.
[16] Cf. E. Fischer-Jørgensen, *loc. cit.* p. 219.

may, by definition, be thought of from the physical standpoint. But words (or morphemes, as the case may be) do not necessarily *name* anything at all, and the linguist is not concerned with whether they do.

It may seem strange that the notion of a "content-substance" turns up, in slightly different guise, in the works of American anti-mentalists.[17] Since meanings, quite rightly, are not regarded as mental entities, it is tempting to regard them as entities of some other sort, e.g. social or physical entities. But meanings are not entities at all; they are the ways in which signs are used.

However, these different metaphysical attitudes have no effect upon linguistic practice. The principle of relevance is everywhere essentially the same. But there is nevertheless a difference of interpretation which, though far less often discussed than those above, has (unlike them) some practical consequences. Many linguists apply the criterion of commutation only to the *arbitrarily* distinctive, disregarding facts which may be treated as synchronically motivated. For others, the distinction between the arbitrary and the motivated appears unimportant, or even (perhaps) without scientific basis. It is indeed rather difficult to tell the present status of this old criterion, which has not been explicitly attacked but which is usually left unmentioned. Nevertheless it is easy to see why its scientific respectability might have suffered within the last thirty years. By its use, the vague notion of "causality" (nowhere any longer a very reputable term) appears to be gratuitously introduced; and this notion seems peculiarly out of place in a science which claims to be *descriptive*.

It is nevertheless very difficult to conceal the suspicion that this criterion is made generous use of. The identification of Japanese ϕ (before *u*) with *h* (before other vowels) is no easier to base on a set of positive features which uniquely define the phoneme than the identification of English (initial) *h* with (non-initial) *ŋ*. Yet the former identification is invariably accepted and the latter invariably rejected. The reason is surely that one cannot see the alternation as *induced* by the environing phones in the latter case.

The word *induce*, still so normal in historical grammar, is sparingly used by phonemicists. But it is used, for instance, by R. Jakobson.[18] It would surely be very implausible to contend that the word has a different meaning in phonemics from that which it has when sound-changes are being described, apart from the difference automatically entailed by the fact that the use is descriptive and not historical. It would surely also be equally implausible to contend that there is something unclear or unscientific about its use to describe the relations of allo-

[17] For instance Zellig S. Harris says that "there are at present no methods of measuring social situations and of uniquely identifying social situations as composed of constituent parts, in such a way that we can divide the utterance which occurs in (or corresponds to) that social situation into segments which will correspond to the constituent parts of the situation" (*Methods in Structural Linguistics*, p. 188–9). As W. Haas rightly answers: "There never will be such methods. The demand for them is preposterous. Speech *engages* in social situations, but its internal structure does not *correspond* to them" (On Defining Linguistic Units; a paper to appear in the 1954 *Transactions of the Philological Society*).

[18] E.g. *Preliminaries*, p. 4.

phones, within the static system, when its use in describing the historical origin of allophones is perfectly intelligible.

One might recognize this and yet draw another conclusion: *not* that the notion is essential in synchronic linguistics, but that it would better be dispensed with in *historical* linguistics. One would indeed have supposed that its use in linguistic reconstruction was central, even if it could be disregarded in the actual statements about the evolution of the systems. But this also has been disputed.

Linguistic reconstruction is doubly relevant to the point at issue. Firstly it has always been recognized that only phonemic distinctions afford a basis for reconstruction. Secondly, there is an analogy between the complementary distribution of allophones as a basis for their identification in the system of one language, and the complementary distribution of phonemes, as between one language and another, as a basis for their identification from the standpoint of the reconstructed system. The latter analogy has recently been stressed by H. M. Hoenigswald.[19]

It is clear that the importance of the phonemic principle for reconstruction, recognized long before the term itself was used, cannot directly derive from the principle of commutation, but only from the principle of the arbitrary. If allophones do not afford a basis for reconstruction, this is due, not to their lack of communicative function, but uniquely to the fact that they are motivated.

If this were not abundantly evident in itself, it would be shown by the instances in which the criteria of relevance and arbitrariness do not coincide. If we did not happen to know the history of English and German, the difference between initial *h* and non-initial ŋ would nevertheless unhesitatingly be used for the purpose of reconstructing earlier periods, since to regard the difference as a motivated variation would not be phonetically plausible. Inversely, nobody has undertaken to reconstruct an "Urintonation" for any types of sentence in Germanic on the basis of comparison between the modern dialects; for the distinctions, though relevant to communication, are in a large degree motivated. If then the difference between the facts which are, and the facts which are not, utilizable in reconstruction, answers to the difference between the phonemic and the non-phonemic, this latter distinction is based directly, not on the principle of relevance, but on the principle of the arbitrary.

It may be thought unfortunate for our argument that features of sentence-stress and sentence-intonation are nowadays often regarded as phonemic. But attempts at analysis based on this view derive of course from the impression that the criterion of relevance is *the* phonemic criterion, not from any *prima facie* feasibility of such analysis. No agreement, even of the most approximate nature, has been reached on the units concerned; and the basic principles involved have been subjected to devastating criticism, most recently by D. L. Bolinger.[20] The

[19] H. M. Hoenigswald, The Principal Step in Comparative Grammar, *Language* 26.357–364 (1950). It is interesting that Hoenigswald only refers once to the criterion of phonetic plausibility in comparative grammar (p. 363), and does not refer at all to its equivalent in synchronic phonemics.

[20] Intonation: Levels versus Configurations, *Word* 7.199–210 (1951).

proposal of C. F. Hockett[21] to regard differences of *speed of utterance* as phonemic, appears not to have given rise to any discussion. The proposal is of course only logical, if mere communicative relevance is a sufficient criterion. Even speed of utterance may communicate something.

Direct appeals from the principle of relevance to the principle of the arbitrary are not uncommon so far as sentence-features are concerned. They are however extremely rare in the traditional domain of phonemics. Quite exceptional is the explicit statement[22] of A. Martinet: "Two phonemes can only be said to belong to the same order if they both present the local characteristic articulation in exactly the same form or forms which deviate from each other only through features which can be fully accounted for as due to the synchronic influence of a concomitant articulation". The statement can surely be generalized to cover, not only the classification (or feature-analysis) of phonemes, but also the analysis of larger units into phonemes. It is the implicit application of such a criterion that renders inacceptable, to the vast majority of linguists, the analysis of English "long vowels" into units composed of a short vowel plus *h*.

However the same criterion may be said to be as good as explicit in the works of such a scholar as Martin Joos.[23] His term *neureme* appears to have no other empirical basis than the feasibility of explaining variants in terms of environment. His speculations on the cerebral correlates of the phonemes do not seem to have any firmer scientific basis than the (curiously similar) speculations of Hermann Paul on the cerebral correlates of morphological categories. But his insistence on the importance of *explaining* allophonic variations remains significant.

The phonemes are the arbitrary residue left after the deduction ("discounting") of whatever is to be regarded as motivated. (The principle involved may indifferently be described as the "principle of the arbitrary" or the "principle of motivation.") The criterion of the "common arbitrary basis" is a meta-criterion for weighing the rival claims of the criteria of intrinsic identity and distribution. Neither of these criteria has any initial priority. The aim is to reach a system whereby intrinsic features and distribution are *mutually explanatory*. Of course there is always the possibility that several different systems may serve this purpose equally well. But this would be a poor argument for regarding with leniency systems which do not fulfill the purpose at all.

In morphemics the conflict between the criterion of commutation and the criterion of the arbitrary has far more often been the subject of discussion. Linguists who cling firmly to the principle of commutation will present such analyses as that of French *dépenser* into *dé-penser*,[24] on the basis of such pro-

[21] Peiping Morphophonemics, *Language* 26.76–77 (1950).

[22] Function, Structure, and Sound Change, *Word* 8.16ff. (1952).

[23] *Acoustic Phonetics* (Language Monograph No. 23, 1948).

[24] The example is taken from K. Togeby, *Structure immanente de la langue française*, Copenhagen 1951, p. 231. The objection to such analyses might be expressed by saying that they do not answer to the criterion of *factually frequent commutation*. Frequent utterances containing *penser* do not usually answer to frequent (and otherwise identical) utterances containing *dépenser*. One may of course agree with the glossematists that frequency is a

portions as *disposer* : *déposer* = *dispenser* : *dépenser*; while linguists for whom the principle of the arbitrary is essential, object that the form in question is not synchronically motivated and hence cannot be further analyzed. Unfortunately the defense of the latter standpoint has frequently been vitiated by references to "speech-feeling," so that the impression may be left that the view is merely subjective. But the objective grounds are quite clear. *Penser* and *dépenser* are seldom used in the same *verbal* contexts, as could easily be shown by a statistical investigation if anybody were seriously concerned to deny the fact.

The object of linguistic analysis is the establishment of the minimal arbitrary code on the basis of which the facts of speech may be understood. A phonemic analysis is unsatisfactory if the allophonic variants cannot be *understood* in terms of environment; it is not enough that they should be merely *predictable* in such terms. Similarly a morphemic analysis is unsatisfactory if it does not explain the *use* of the units analyzed; or more strictly, if it does not provide the basis of arbitrary tradition on which, once all contextual perturbations have been allowed for, this use can be understood.

While the criterion of commutation or non-redundancy is bound up with the principle of economy of *inventory*, the criterion of the arbitrary is bound up with the principle of economy of *explanation*. It may surely be agreed that the latter principle is more important. If a reduction in inventory renders the facts of speech more difficult to explain, it is worse than valueless. The criterion of non-redundancy is not a criterion in its own right; its value in phonemics is due to the fact that the redundant is often at the same time the motivated. Whenever the two criteria do not coincide, its secondary status becomes apparent.[25]

University of Istanbul

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matter of *usage* alone, not of linguistic *system*. But it would be mistaken to conclude from this that frequency can be disregarded in the process of constructing the system. The system is constructed so as to account for all the facts of speech, including those facts which are not recorded in the formulation of the code as such.

[25] In this connection, see the last paragraph of A. Martinet, Non-Apophonic *O*-Vocalism in Indo-European, *Word* 9.253–267 (1953), p. 267.

CRITÈRES DE DÉLIMITATION[1]

HENRI FREI

«Pour échapper aux illusions, il faut d'abord se convaincre que les entités concrètes de la langue ne se présentent pas d'elles-mêmes à notre observation. Qu'on cherche à les saisir, et l'on prendra contact avec le réel; partant de là, on pourra élaborer tous les classements dont la linguistique a besoin pour ordonner les faits de son ressort.»

F. de Saussure.[2]

1. LA MÉTHODE DE DÉLIMITATION DE SAUSSURE repose sur le critère de la substitution, en liaison avec la considération du signifié.[3]

«Soit en français *sižlaprã*: puis-je couper cette chaîne après *l* et poser *sižl* comme unité? Non: il suffit de considérer les concepts[4] pour voir que cette division est fausse. [. . .] Les seules divisions possibles sont: 1° *si-ž-la-prã* («si je la prends»), et 2° *si-ž-l-aprã* («si je l'apprends»), et elles sont déterminées par le sens qu'on attache à ces paroles.

«Pour vérifier le résultat de cette opération, et s'assurer qu'on a bien affaire à une unité, il faut qu'en comparant une série de phrases où la même unité se rencontre, on puisse dans chaque cas séparer celle-ci du reste du contexte en constatant que le sens autorise cette délimitation.»[5]

Jusqu'à ce jour, quarante ans après l'enseignement de Saussure, les linguistes n'ont pas encore réussi à découvrir une méthode qui permettrait de délimiter les monèmes[6] sans tenir compte du signifié.

2. LES MARQUES DE DÉLIMITATION, c'est-à-dire les faits phoniques, et spécialement prosodiques, qui contribuent, dans beaucoup de langues, à signaler les

[1] Robert Godel et Edmond Sollberger ont bien voulu lire une ébauche de cet article' fragment d'un essai en préparation (*La notion de monème*).

Je me réfère aux publications selon le système adopté par le Comité International Permanent de Linguistes pour ses volumes annuels de *Bibliographie Linguistique*, Utrecht et Bruxelles, 1949 et sv.

[2] *Cours de linguistique générale*, chap. *Identités, réalités, valeurs*, §B. (A cause des différences de pagination des éditions, je renvoie aux titres de chapitres et aux paragraphes).

[3] J'appelle *signifié* celle des deux faces du signe qui ne peut être réalisée physiquement (dans la parole), *signifiant* celle qui le peut.

[4] D'après les notes des étudiants, l'introduction des termes *signifiant* et *signifié* est postérieure à ce passage; ils n'apparaissent qu'après le 9 mai 1911. Mais l'emploi d'autres termes ne change rien à la pensée de l'auteur, dont les théories étaient fixées depuis longtemps.

[5] Chap. *Les entités concrètes de la langue*, §2.

[6] «J'appelle monème un signe dont le signifiant est insécable, c'est-à-dire n'est pas divisible en signifiants [successifs] plus petits. Dans le syntagme *poirier*, les éléments *poir-* et *-ier* sont des monèmes.» (ZPhon IV 1950, 162, n. 4). Cf. E. Sollberger, *Note sur l'unité linguistique* (CFS XI 1953, 45–46). A cause de l'inclusion du signifié, cette notion ne se recouvre donc pas avec celle de *morpheme*, *morpheme alternant* ou *morph*, termes qui sont d'ailleurs en étroit rapport avec le concept équivoque de *morphologie*, que la linguistique saussurienne rejette (cf. Saussure, chap. *La grammaire et ses subdivisions*, §1).

limites de monèmes et de syntagmes, ont un caractère trop sporadique pour pouvoir être utilisés autrement qu'à titre de critère accessoire.

Ainsi les pauses, réelles ou potentielles,[7] qui, en pratique, ne tombent guère au milieu du monème, servent bien à déceler le début et la fin des autophones[8] (*Moi. Lui. Aussi. Non. Chez⌒lui. Sa⌒soeur. Il⌒ira.*), mais ne constituent pas un critère décisif pour la délimitation des symphones, puisque ceux-ci, tantôt ne sont jamais précédés d'une pause (pren-*ez*, désir-*eux*), tantôt ne précèdent jamais une pause (*dé*-faire, *buv*-eur), tantôt possèdent ces deux caractères à la fois (mang-*i*-ez, mange-*r*-ez, vis-*ibil*-ité).

3. Le critère analogique. Saussure a signalé l'importance des formations analogiques comme critère de délimitation: «[. . .] si les unités vivantes, ressenties par les sujets parlants à un moment donné, peuvent seules donner naissance à des formations analogiques, réciproquement toute répartition déterminée d'unités suppose la possibilité d'en étendre l'usage. L'analogie est donc la preuve péremptoire qu'un élément formatif existe à un moment donné comme unité significative.»[9]

Cette valeur d'indice de l'analogie a été souvent réaffirmée, et chez Dwight L. Bolinger elle fait même partie de la définition de l'unité: «a morpheme is a least element that can enter into new combinations.»[10]

3.1. [Analogie et synchronie.] Zellig S. Harris, en revanche, exclut les formations analogiques: «In the latter case [*de-frost, de-icer, We better re-polish it*], one can term the morphemes, especially the bound morphemes, which occur in new combinations 'productive'. However, the methods of descriptive linguistics cannot treat of the degree of productivity of elements, since that is a measure of the difference between our corpus (which may include the whole present language) and some future corpus of the language. If we wish the analysis of our corpus to differ as little as possible from the corresponding part of any other corpus of the language, now or in the near future, i.e. if we wish our statements about the corpus to be predictive for the language, we must devise our approximation of morpheme classification in such a way as would disregard the variations and innovations noted here.»[11] Ce point de vue appelle plusieurs réserves.

D'abord, à considérer les exemples cités, ce ne sont pas les *morphemes* qui sont productifs, mais leurs combinaisons, que la linguistique saussurienne appelle des *syntagmes*. Un monème ne saurait être productif qu'en tant qu'agencement de phonèmes servant de modèle à un nouveau monème. On peut supposer que lorsque George Eastman créa le mot *kodak*, il le fit sur le modèle de monèmes présentant le schème /cvcvc/.

Ensuite, le discours, qui correspond aux rapports syntagmatiques ou *in praesentia* de Saussure, et qui ne peut, ni fonctionner ni être interprété par le

[7] C. F. Hockett, IJAL XIV 1948, 2. A. W. de Groot, Word IX 1953, 63.

[8] H. Frei, *Cas et dèses en français*, §2.2. (CFS XII 1954).

[9] Chap. *Analogie et évolution*, §2.

[10] *On Defining the Morpheme*, Word IV 1948, 21. Cf. aussi H. Rubenstein, Lang XXVIII 1952, 115–116.

[11] *Methods in Structural Linguistics*, Chicago 1951, 255 (Appendix to 15.2).

linguiste sans les rapports associatifs ou *in absentia* correspondants,[12] est une manifestation du système linguistique, mais il n'en est jamais qu'une manifestation partielle. Il est donc possible que deux *corpus* différents, c'est-à-dire deux collections de matériaux prélevés dans le discours, reflètent le même système. Or, le but essentiel du linguiste n'est pas l'étude exclusive du *corpus*, qui ne pourrait aboutir qu'à un inventaire, mais celle du système à travers le *corpus*. Ce qui l'intéresse, par exemple, dans une formation analogique telle que *indécorable*,[13] ce n'est pas ce néologisme en soi, mais le fait qu'il révèle que d'autres signes, *décorer*, *décoration*, etc., *pardonnable*, *maniable*, etc., *inconnu*, *insensé*, etc., sont analysés *décor-er*, *décor-ation*, etc., *pardonn-able*, *mani-able*, etc., *inconnu*, *in-sensé*, etc. Heureusement, Harris lui-même, au début de son ouvrage, autorise le linguiste à élargir son *corpus* en cours de route: «The corpus does not, of course, have to be closed before analysis begins. Recording and analysis can be interwoven, and one of the chief advantages of working with native speakers over working with written texts (as is unavoidable, for example, in the case of languages no longer spoken) is the opportunity to check forms, to get utterances repeated, to test the productivity of particular morphemic relations, and so on. [Footnote 12: If the linguist has in his corpus *ax*, *bx*, but not *cx* (where *a*, *b*, *c* are elements with general distributional similarity), he may wish to check with the informant whether *cx* occurs at all.]»[14] N'est-ce pas là, au fond, un aveu d'impuissance, impuissance à saisir le système à travers un *corpus* limité?

Un *corpus* qui contient des formations analogiques n'est donc pas nécessairement, par rapport à un *corpus* qui ne présente que le matériel traditionnel, un «future corpus» ou «in the near future», et l'on se demande si la «descriptive linguistics» s'est entièrement débarrassée du virus diachronique: «néologique» et «futur» ne sont pas plus des synonymes que «archaïque» et «ancien».[15] Enfin, l'auteur n'a évidemment pas songé au fait que très souvent les formations analogiques et les formations traditionnelles correspondantes (quand elles existent) se rencontrent simultanément dans un même *corpus*: celui qui laisse échapper *vous disez* ou *vous faisez* peut très bien, l'instant d'après, employer la forme correcte *vous dites* ou *vous faites*.

3.2. [Analogie productive ou reproductrice.] Quand on parle de formations analogiques, il importe d'ailleurs de faire une distinction entre l'analogie productive, qui crée des syntagmes nouveaux (plais-ons : plais-ez :: fais-ons : *fais-ez*), et l'analogie reproductrice, qui se contente de reformer des syntagmes déjà existants (dīc-it : dīc-unt :: ag-it : *ag-unt*). Saussure[16] a montré, en effet, que

[12] Chapitres *Rapports syntagmatiques et rapports associatifs* et *Mécanisme de la langue*.

[13] Saussure, chap. *L'analogie*, §3.

[14] *Methods in Structural Linguistics*, 12 (§2.33).

[15] Cf. Saussure, chap. *La langue la plus ancienne et la langue primitive*, 3°.—Cette compartimentation arbitraire rappelle, dans un autre domaine, l'obligation où se croient les dialectologues de choisir comme témoin l'homme le plus âgé et le plus sédentaire d'un village afin de fixer le parler sous sa forme historiquement la plus pure, comme si dans la réalité courante, autrefois comme aujourd'hui, tous les sujets n'étaient pas au fond bilingues aux degrés les plus divers.

[16] Chap. *Analogie et évolution*, §3.

l'analogie «intervient non seulement quand des matériaux préexistants sont distribués dans de nouvelles unités, mais aussi quand les formes restent identiques à elles-mêmes», et que «son principe est au fond identique à celui du mécanisme du langage»: dans les deux cas il s'agit d'obtenir au fur et à mesure, sur le modèle des syntagmes tenus en réserve dans le système, ceux dont le discours a besoin.[17]

3.3. [Présence indispensable du signifié dans le critère analogique.] Que l'on ait affaire à l'analogie productive ou à l'analogie reproductrice, l'analyse sématique[18] et l'analyse sémantique des termes mis en rapport doivent coïncider. Une proportion telle que *ōrātōr-em* : *ōrātor* :: *honōr-em* : *honor* «n'aurait aucune raison d'être si l'esprit n'associait pas par leur sens les formes qui la composent.»[19]

Il s'ensuit que l'analogie productive est un critère de délimitation plus sûr que l'analogie reproductrice. Elle indique que le découpage est tel qu'on se l'imagine ou qu'en l'espèce un parleur donné le conçoit autrement, tandis que, dans le cas de la seconde, on ne sait pas toujours jusqu'à quel point les formules proportionnelles et les paradigmes que construit le linguiste sont en accord avec la correspondance signifiant-signifié.

A propos de la décomposition, contestée, de *deceive* en *de-* et *-ceive*, faite par Eugene A. Nida et d'autres,[20] André Martinet écrit : «To some scholars, it would seem at first blush that e.g. *deceive* cannot by any means be analyzed into two successive semantic units, and that, from the point of view of contemporary English as spoken by millions who have not the vaguest idea of the etymology of the word, its rhyming with *receive* and *conceive* is a pure matter of chance. Yet, some kind of formal analysis is undoubtedly supported by a very large number of such proportions as *deceive, deceit, deception* : *receive, receipt, reception* : *conceive, conceit, conception*; and *deceive, depress, depose* : *receive, repress, repose* : *conceive, compress, compose.*»[21] Mais alors que dans une proportion telle que *dīc-it* : *dīc-unt* :: *ag-it* : *ag-unt* le signifiant et le signifié sont exactement symétriques ('il dit' : 'ils disent' :: 'il conduit' : 'ils conduisent'), qu'en est-il dans le cas de *deceive*? Peut-on dire que 'tromper' est à 'concevoir' ce que 'tromperie' (*deceit*) est à 'vanité' (*conceit*), ou que 'tromper' est à 'recevoir' ce que 'abaisser' ou 'abattre' (*depress*) est à 'réprimer' (*repress*), etc.? Des néologismes comme **beceive*, **counterceive*, **outceive*, **overceive*, **unceive*, **underceive*, **withceive*, etc., s'ils existaient, auraient une autre valeur probante que cette «formal analysis», qui est justement formelle au sens propre du terme. Sur la même base, on pourrait tout aussi bien essayer de prouver que le radical du verbe *apprendre* se décompose en /a/ + /prād/, puisqu'on a *peler* : *appeler*, *river* : *arriver*, *teindre* : *atteindre*, etc.

Pour la même raison exactement, *become* et *begin*, que Bernard Bloch, dans

[17] La limite entre les deux types est quelquefois difficile à établir: *draftee*, qui se trouve chez Webster (*College edition*, 1953), mais non chez Harrap (1953), est peut-être pour beaucoup d'Américains un simple cas d'analogie reproductrice et pour beaucoup d'Anglais un néologisme.

[18] J'appelle *sématique* tout ce qui concerne le signifiant.

[19] Saussure, chap. *L'analogie*, §3.

[20] E. Nida, *Morphology*, Ann Arbor² 1949, 162, 191. De même: B. Bloch & G. L. Trager, *Outline of Linguistic Analysis*, Baltimore 1942, §4.4.

[21] Word VI 1950, 86.

une étude de linguistique synchronique, classe curieusement parmi les «derivative verbs formed with a prefix», respectivement parmi les verbes «where a given base does not occur without a prefix»,[22] sont des monèmes. Le sens ne permet pas d'introduire dans la série proportionnelle *fall* : *be-fall* :: *moan* : *be-moan* :: *stride* : *be-stride*, etc., le couple mal assorti *come* : *become*, ni le couple inexistant **gin* : *begin*.

4. Le critère distributionnel a pour objet, chez certains linguistes, de remplacer la définition et la délimitation sémantiques des monèmes par leur répartition dans la chaîne du discours: «Within an English morpheme some sequences of phonemes are forbidden. For example, what is commonly spelled *ou* (as in *council*) is never followed by *p*, *b*, *f*, *v*, or *m*. [. . .] analogous restrictions apply at every level of analysis up to the complete utterance. Above the level of the phonemic system, these restrictions define for us the "grammar" of the language, and all without reference to what is popularly thought of as the "meaning",[23] namely the popular categorizations of continuous reality [. . .], just as we find it expedient to define the phonemic system independently of the phonetician's categorizations of continuous speech.»[24]

4.1. [Inexhaustivité.] Si l'on veut admettre que le critère distributionnel est exact, et le seul possible pour l'identification et la délimitation des monèmes que renferme un état de langue, il s'ensuit, à priori, que chacun de ceux-ci doit se rencontrer dans une position au moins dont tous les autres sont exclus.

La vérification vaudrait la peine d'être tentée. Pour ne citer, provisoirement, qu'un seul exemple, il semble que les noms propres qui font partie d'une classe donnée passent entre les mailles du filet. Ainsi, aucun prénom ne peut figurer dans telle ou telle combinaison sans qu'un autre ne puisse s'y trouver aussi: *Charles* (*Jean*, *Pierre*, etc.) est venu; J'ai vu la soeur de *Charles* (*Jean*, *Pierre*, etc.), etc. On pourrait objecter qu'un même prénom ne saurait être répété, sauf situations spéciales (homonymie), dans des exemples tels que: **Charles* parle à *Charles*, ou: **Charles* a rendu le livre à *Charles*, ou encore: **Charles* est le frère de *Charles*. Mais cette impossibilité, la même qui interdit de dire: **Cette table ronde est carrée*, n'est pas d'ordre grammatical. En acceptant cette objection, on chuterait dans «the real outside world»,[25] que les distributionalistes,[26] les saussuriens (parole!) et les glossématistes (substance!) prétendent également exclure.

4.2. [Parenté avec le critère analogique.] Par le fait que les formations analogiques, qu'il s'agisse de l'analogie productive ou reproductrice, ne sont «qu'un aspect du phénomène d'interprétation, une manifestation de l'activité générale qui distingue les unités pour les utiliser ensuite»,[27] le critère distribu-

[22] Lang XXIII 1947, 409.

[23] L'idée qu'on se fait, populairement, du *sens* ou de la *signification* (angl. *meaning*) est soit le contenu de conscience, soit le référé (*referent*, *thing-meant*). Le signifié saussurien est autre chose: cf. Saussure, chap. *La valeur linguistique*, §2.

[24] M. Joos, JAcS XXII 1950, 708.

[25] Joos, *ibid.*, 704.

[26] Joos, *ibid.*, 703: «Let sociologists and others do what they like with such things—».

[27] Saussure, chap. *L'analogie*, §3.

tionnel s'apparente au critère analogique. La plupart des raisonnements formulés en termes de distribution pourraient être transposés en proportions analogiques, et inversement. Ainsi, au lieu de définir l'élément *dé-* d'une manière distributionnelle, par exemple comme un symphone préverbal, on peut aussi dire qu'un syntagme en *dé-* fait partie d'une proportion telle que *dé-faire* : *re-faire* :: *dé-coudre* : *re-coudre* :: *dé-boiser* : *re-boiser*, etc.

L'opposition de ces deux méthodes, comme l'a montré Saussure, se reflète «dans deux doctrines grammaticales différentes. Nos grammaires européennes opèrent sur la quatrième proportionnelle; elles expliquent par exemple la formation d'un prétérit allemand en partant de mots complets; on dit à l'élève: sur le modèle de *setzen* : *setzte*, formez le prétérit de *lachen*, etc. Au contraire la grammaire hindoue étudierait dans un chapitre déterminé les racines (*setz-*, *lach-*, etc.), dans un autre les terminaisons du prétérit (*-te*, etc.); elle donnerait les éléments résultant de l'analyse, et on aurait à recomposer les mots complets.»[28]

4.3. [Présence indispensable du signifié dans le critère distributionnel.] Le problème de *ceive* (§3.3) a été posé par Harris, indépendamment de Martinet, en termes distributionnels (d'ailleurs mêlés de proportions analogiques): «A more complicated case occurs in the pairs of words or utterances *conceive–receive*, *concur–recur*, *confer–refer*, etc. We never get *ceive* by itself, but every phoneme sequence with which *ceive* occurs, appears also with other bound forms [Footnote 9: I.e. with other sequences which don't occur by themselves] which in turn occur with one or more of the sequences with which *ceive* occurs: *perceive*, *deceive*; *deduct*, *conduct*; *perjure*, *conjure*; *persist*, *desist*, *consist*, *resist*, *assist*. There is thus discovered a family of initial bound forms (prefixes) and a family of non-initial bound forms (stems), between the members of which families this relation in general holds. Each one of these prefixes occurs with several of these stems, and vice versa. This gives us a preview of the exact relation which may obtain among these sequences if we break them up in the above manner. On the basis of such a preview we decide that it is worth considering *con-*, *re-*, *-ceive*, *-cur*, etc., as each a distinct morphemic segment. [Footnote 10: The two sets *con-*, *re-*, *per-*, etc., and *-ceive*, *-cur*, *-sist*, etc., are dependent on each other as sets; but any one member of the first set is not dependent on any one member of the second.]»[29]

Appliquée véritablement à la lettre, c'est-à-dire sans considération du signifié, comme l'exige l'auteur,[30] cette manipulation (tenu compte de la restriction de la note 10) risquerait de produire des résultats cocasses. Ainsi l'on pourrait poser, pour le français, d'une part une famille de préverbes (*dé-*, *é-*, *en-*, *par-*, *ré-*, etc.), d'autre part une famille de radicaux verbaux symphones par l'initiale, par exemple *-tage* (extrait de *étager* et *partager*), *-tale* (extrait de *détaler* et *étaler*), *-tame* (extrait de *entamer* et *rétamer*), etc.

[28] *Ibid.*

[29] *Methods in Structural Linguistics*, 161 (§12.232).

[30] «In determining the morphemes of a particular language, linguists use, in addition to distributional criteria, also (in varying degrees) criteria of meaning difference. In exact descriptive linguistic work, however, such considerations of meaning can only be used heuristically, as a source of hints, and the determining criteria will always have to be stated in distributional terms (Appendix to 12.41). The methods presented in the preceding chapters offer distributional investigations as alternatives to meaning considerations» (365, n. 6).

Si le résultat n'est en général pas aussi ahurissant, c'est parce que le distributionaliste, *nolens volens*, tient néanmoins compte du signifié, au moins «as a source of hints», ou plus exactement comme un garde-fou. Il est douteux qu'il soit absent du raisonnement de Harris et qu'il n'en fasse pas partie subrepticement: *Naturam expelles furca, . . .* L'auteur connaît évidemment par leurs signifiés l'existence de préverbes tels que *con-*, *de-*, *per-*, *re-*, etc., dans des syntagmes où ils sont des monèmes légitimes (*center–concenter*, *frost–defrost*, *ambulate–perambulate*, *elect–reelect*, etc.). Qu'est-ce qui empêcherait, sinon, de couper *conc-eive* et *cons-ist*, *rec-eive* et *res-ist*, etc., de poser des «stems» *-eive* et *-ist*, et de considérer *con-* (dans *concur*, *confer*, etc.) et *cons-*, respectivement *re-*, *rec-* et *res-*, etc., comme des variétés («alternants») préconsonantiques et prévocaliques?

4.4. [Non-unicité et atomisme.] Nous rencontrons ici une difficulté méthodologique qui a été signalée déjà dans le domaine de la phonématique:[31] la non-unicité des solutions possibles.[32] Alors qu'un critère pertinent, appliqué correctement, doit à priori n'autoriser qu'une solution pour chaque problème, le critère distributionnel, s'il est employé exclusivement sans considération du signifié, permet en théorie, pour n'importe quel échantillon de chaîne parlée, n'importe quelle délimitation, sans autres restrictions que celles qui sont imposées individuellement par le degré d'imagination du savant.[33]

La non-unicité des solutions possibles est liée à l'atomisme. Dans la mesure où il forme un système cohérent, un état de langue n'autorise pas plusieurs réponses par problème, et, comme dans un jeu de mots croisés, toute solution fausse entraîne automatiquement la fausseté des autres. Admettre la non-unicité des solutions, c'est nier la notion de système linguistique; comme tout se tient, on devine pourquoi les distributionalistes s'intéressent au fond si peu à ce concept.

4.5. [Circularité.] On affirme que les phonèmes ne se répartissent pas sur la ligne du discours de n'importe quelle manière, ce qui est exact, au moins en partie.[34] Mais ceux-ci, comme cela ressort de la pratique des distributionalistes eux-mêmes, ne peuvent être délimités et définis qu'en fonction des monèmes et des syntagmes. On se meut donc dans un cercle vicieux lorsqu'on prétend déterminer les unités significatives par la répartition des phonèmes qui les composent.

D'une manière plus générale encore, le critère distributionnel est soumis à un axiome,[35] que l'on pourrait formuler ainsi: Toute distribution implique des éléments. Or, la langue ne connaît pas d'éléments préétablis. Les phonèmes et les signifiants ne sont pas des données de la chaîne sonore, mais des découpures pratiquées par la langue dans ce continu physique. Celle-ci, d'après Saussure,[36]

[31] Y. R. Chao, *The Non-Uniqueness of Phonemic Solutions of Phonetic Systems*, BIHP IV 1934, 363–397.

[32] Harris l'admet ingénument: «The methods described here do not eliminate non-uniqueness in linguistic descriptions» (*ibid.*, p. 2).

[33] On aurait donc tort, comme on serait tenté de le faire, d'assimiler les distributionalistes à des joueurs de mots croisés. Un «mots croisés», comme, en principe, un système linguistique, n'admet qu'une réponse par question.

[34] Pour cette réserve, cf. Martinet, Word IX 1953, 79, et pour la même réserve concernant la répartition des monèmes, cf. *supra*, §4.1.

[35] Jamais exprimé, sauf défaut d'information de ma part.

[36] Chap. *La valeur linguistique*, §1.

«élabore ses unités en se constituant entre deux masses amorphes»: «le plan indéfini des idées confuses et [. . .] celui non moins indéterminé des sons»;[37] et «*cette combinaison produit une forme, non une substance.*» Il faut en conclure que le critère distributionnel ne pourrait être appliqué exclusivement que si la langue était une substance, composée d'éléments donnés d'avance, car une définition distributionnelle d'éléments non préétablis ne saurait être que circulaire, le défini (*éléments*), selon l'axiome ci-dessus, étant inclus dans le définissant (*distribution*).

4.6. [La linguistique une mathématique ?] Einar Haugen ne semble pas prendre au tragique la circularité souvent reprochée aux distributionalistes: «It has been a constant charge brought against the metalinguists[38] that their procedures have seemed circular. [. . .] Perhaps we may recognize now that this is inherent in the adoption of distributional criteria. The resulting circularity is probably no more a fault in linguistics than it is in mathematics. Mathematics is also circular in that it does not depend for its validity on the existence of any particular kind of reality.»[39]

Si cela était, la linguistique ainsi comprise serait, sauf erreur, la seule discipline, en dehors des mathématiques, qui occupent une place à part dans la classification des sciences, à faire du cercle vicieux la base même de sa méthode.

Mais les notions de *circulaire* et d'*irréel* ne sont pas nécessairement synonymes. Ainsi, si le signifiant (avec le phonème) et le signifié sont tous les deux des entités non préétablies, donc irréelles, en ce qu'elles ne sont données ni dans la chaîne acoustique ni dans la pensée, la définition de l'un par l'autre n'implique pas circularité, puisque, même si l'on peut admettre avec Saussure que dans le discours ils se correspondent un à un,[40] ils diffèrent non seulement par leur matière (critère extra-linguistique), mais encore par l'absence d'isomorphisme entre les parties qui les constituent: d'une part les phomèmes et leurs composantes (*distinctive features*),[41] d'autre part les composantes sémantiques.[42]

[37] Martin Joos soutient cette thèse à son tour, 40 ans après Saussure et apparemment sans le connaître: *Description of Language Design*, JAcS XXII 1950, 701–708. Joos considère chacun de ces deux plans comme continu: cf. le passage cité *supra*, §4.

[38] Chez G. L. Trager, ce terme désigne au contraire les sémantistes.

[39] Lang XXVII 1951, 221.

[40] Chap. *Les entités concrètes de la langue*, §2: «Une délimitation correcte exige que les divisions établies dans la chaîne acoustique [. . .] correspondent à celles de la chaîne des concepts.» Cf. chap. *La valeur linguistique*, §1: «La langue est encore comparable à une feuille de papier: la pensée est le recto et le son le verso; on ne peut découper le recto sans découper en même temps le verso; [. . .]». Cette biunivocité a été souvent contestée: cf. Rulon Wells, Lang XXVII 1951, 561 (*breakdown of isomorphism*), et Ch. Bally, *Linguistique générale et linguistique française*, Berne[2] 1944, §§217–272 (*dystaxie*), dont le point de vue s'explique malheureusement en grande partie par l'ignorance de la distinction entre signifié et composantes du signifié.

[41] Roman Jakobson, C. Gunnar M. Fant, Morris Halle, *Preliminaries to Speech Analysis*, Massachusetts Institute of Technology, Technical Report No. 13, [Cambridge, Mass.] 1952.

[42] R. Jakobson, *Actes du Sixième Congrès International des Linguistse*, Paris 1949, 7 (*semantic minimums*). H. Frei, *Langue, parole et différenciation*, JPsych XLV 1952, §§2.3 (*variétés sémantiques*), 3.2.3 (*subsignifiés*). W. L. Wonderly, *Semantic Components in Kechua Person Morphemes*, Lang XXVIII 1952, 366–76.

Et les entités avec lesquelles opère le linguiste actuel, quoiqu'il ne les trouve pas préfabriquées dans les ondes sonores ou dans le courant de la conscience, n'en ont pas moins une certaine consistance. Ce ne sont pas des constructions mathématiques, mais des réalités *sui generis*, dont l'existence se révèle à divers indices externes. On a signalé depuis longtemps que dans l'écriture d'une langue les lettres et leurs combinaisons correspondent souvent mieux aux phonèmes qu'aux sons de celle-ci. Le français n'a qu'un seul phonème /r/, qui se prononce de plusieurs façons: le système graphique du français ne possède qu'une espèce *r*. D'un autre côté, les monèmes d'une langue peuvent être groupés en des entités plus abstraites, les sémions;[42] par exemple, dans la conjugaison du verbe *lever*, les monèmes /ləv/ et /lɛv/ constituent un même sémion. Or, Josef Vachek a remarqué que l'orthographe anglaise s'intéresse plus aux sémions qu'aux monèmes:[44]

house /haus/, *house*-s /ˈhauz-iz/
vari-ous /ˈvɛəri-əs/, *vari*-ety /vəˈrai-əti/
want-*ed* /ˈwɔnt-id/, call-*ed* /kɔ:l-d/, pass-*ed* /pɑ:s-t/,

observation qui se vérifie peut-être pour beaucoup d'autres langues possédant une écriture.

Il y a d'ailleurs une certaine équivoque, même s'il ne s'agissait que de métaphores boursouflées, à parler d'une «algèbre linguistique»;[45] à prétendre que la linguistique est entrée dans sa «mathematical phase»;[46] à affirmer que «of all the sciences and near-sciences which deal with human behavior, linguistics is the only one which is in a fair way to becoming completely mathematical»;[47] à recommander: «within our field we must adopt a technique of precise treatment, which is by definition a mathematics. We must make our "linguistics" a kind of mathematics, within which inconsistency is by definition impossible»;[48] en particulier, à stipuler «the mathematics called "linguistics"» et, dans la phrase qui suit immédiatement, à dire: «Linguistics is a quantum mechanics in the most extreme sense.»[49] Les mathématiques sont les mathématiques, mais la mécanique des quanta est une mécanique, et les physiciens qui s'aident de l'algèbre n'appellent pas leur discipline une algèbre. Autre chose est l'effort d'introduire dans les méthodes de la linguistique l'enviable rigueur des mathématiques,[50] autre chose est l'idée bizarre de vouloir transformer la linguistique elle-même en une mathématique, qui d'ailleurs, d'après les travaux des distributionalistes publiés

[42] Du grec *sēmeîon* 'signe'. La distinction entre monème et sémion correspond, le signifié en moins, à la différence faite par les distributionalistes entre *morpheme alternants* et *morpheme units* (Harris) ou, selon la terminologie plus récente et plus simple de Hockett, entre *morphs* et *morphemes*.

[44] AL V 1945–49, 93.

[45] L. Hjelmslev, *Omkring Sprogteoriens Grundlæggelse*, Copenhague 1943, 72.

[46] Joos, JAcS XXII 1950, 702.

[47] Joos, *ibid.*, 702.

[48] Joos, *ibid.*, 702.

[49] Joos, *ibid.*, 702.

[50] Par exemple par la création d'une notation symbolique (métalangue) inspirée de celle des mathématiques.

jusqu'ici, ne semble pas opérer avec la notion de nombre. Saussure ne songeait pas à faire de la linguistique la caricature d'une autre discipline en proclamant qu'elle «*a pour unique et véritable objet la langue envisagée en elle-même et pour elle-même.*»[51]

4.7. [Délimitation et identification.] Que l'on cherche à délimiter les unités par le critère distributionnel ou que l'on parte d'entités préalablement délimitées pour les définir après coup par ce même critère, la méthode est pareillement circulaire.

Lorsque Martin Joos affirme que «the linguist's "meaning" of a morpheme (or of a combination thereof, [. . .]), is by definition the set of conditional probabilities of its occurrence in context with all other morphemes»,[52] cela suppose que ce signifiant a pu être séparé d'avance des autres signifiants indépendamment des signifiés. Dans l'analyse détaillée à laquelle il a soumis un texte imprimé d'une certaine étendue, Harris[53] reconnaît explicitement cette procédure à deux temps: «The operations make no use of any knowledge concerning the meaning of the morphemes [. . .]. They require only a knowledge of morpheme boundaries, including sentence junctures and other morphemic intonations (or punctuation).»[54] Comment l'auteur aurait-il procédé avec un texte reproduit en transcription phonétique et sans séparations de mots?

On ne peut que renvoyer à l'exemple analysé par Saussure au début de cet article. A l'aide de quels procédés «mathématiques» le distributionaliste décidera-t-il que /sižlaprã/ correspond à *si je la prends* ou à *si je l'apprends*? J'ai bien peur que pour beaucoup de linguistes, tant d'années après Saussure, la délimitation et l'identification des unités ne soient demeurées des opérations qui peuvent être mises bout à bout. Cela rappelle étrangement la division et la succession traditionnelles morphologie–syntaxe, rejetées par le maître genevois[55] et acceptées à peu près sans discussion par des savants qui se croient révolutionnaires.

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[51] Dernière phrase du *Cours de linguistique générale*.
[52] JAcS XXII 1950, 708.
[53] *Discourse Analysis*, Lang XXVIII 1952, 1–30.
[54] *Ibid.*, 30. Cf. pp. 1 et 3 (§1.2., fin).
[55] Chap. *La grammaire et ses subdivisions*, §1.

DISTRIBUTIONAL STRUCTURE

ZELLIG S. HARRIS

1. Does language have a distributional structure? For the purposes of the present discussion, the term structure will be used in the following non-rigorous sense: A set of phonemes or a set of data is structured in respect to some feature, to the extent that we can form in terms of that feature some organized system of statements which describes the members of the set and their interrelations (at least up to some limit of complexity). In this sense, language can be structured in respect to various independent features. And whether it is structured (to more than a trivial extent) in respect to, say, regular historical change, social intercourse, meaning, or distribution—or to what extent it is structured in any of these respects—is a matter decidable by investigation. Here we will discuss how each language can be described in terms of a distributional structure, i.e. in terms of the occurrence of parts (ultimately sounds) relative to other parts, and how this description is complete without intrusion of other features such as history or meaning. It goes without saying that other studies of language—historical, psychological, etc.—are also possible, both in relation to distributional structure and independently of it.

The distribution of an element will be understood as the sum of all its environments. An environment of an element A is an existing array of its co-occurrents, i.e. the other elements, each in a particular position, with which A occurs to yield an utterance. A's co-occurrents in a particular position are called its selection for that position.

1.1. Possibilities of structure for the distributional facts.

To see that there can be a distributional structure we note the following: First, the parts of a language do not occur arbitrarily relative to each other: each element occurs in certain positions relative to certain other elements. The perennial man in the street believes that when he speaks he freely puts together whatever elements have the meanings he intends; but he does so only by choosing members of those classes that regularly occur together, and in the order in which these classes occur.

Second, the restricted distribution of classes persists for all their occurrences; the restrictions are not disregarded arbitrarily, e.g. for semantic needs. Some logicians, for example, have considered that an exact distributional description of natural languages is impossible because of their inherent vagueness. This is not quite the case. All elements in a language can be grouped into classes whose relative occurrence can be stated exactly. However, for the occurrence of a particular member of one class relative to a particular member of another class it would be necessary to speak in terms of probability, based on the frequency of that occurrence in a sample.

Third, it is possible to state the occurrence of any element relative to any other element, to the degree of exactness indicated above, so that distributional state-

ments can cover all the material of a language, without requiring support from other types of information. At various times it has been thought that one could only state the normative rules of grammar (e.g. because colloquial departures from these were irregular), or the rules for a standard dialect but not for "substandard" speech or slang; or that distributional statements had to be amplified by historical derivation (e.g. because the earlier form of the language was somehow more regular). However, in all dialects studied it has been possible to find elements having regularities of occurrence; and while historical derivation can be studied both independently and in relation to the distribution of elements,[1] it is always also possible to state the relative occurrence of elements without reference to their history (i.e. "descriptively").

Fourth, the restrictions on relative occurrence of each element are described most simply by a network of interrelated statements, certain of them being put in terms of the results of certain others, rather than by a simple measurement of the total restriction on each element separately. Some engineers and mathematicians (as also phoneticians and experimental psychologists) who have become interested in language have sought a direct formulation of the total restrictions on occurrence for each element, say for each sound.[2] This would yield an expression for how the occurrences of each element depart from equiprobability, and so would give a complete description of the occurrences of elements in the language. Now it is of course possible to enumerate the relative occurrences of a finite set of elements in finitely long utterances; but direct enumeration is of little interest because it yields no simple description of the over-all occurrences of elements, and because it does not order the restrictions in such a way that the larger restrictions get stated before the smaller ones. In contrast with this, it is possible to describe the occurrence of each element indirectly, by successive groupings into sets, in such a way that the total statements about the groupings of elements into sets and the relative occurrence of the sets are fewer and simpler than the total statements about the relative occurrence of each element directly.

We obtain then an ordered set of statements in terms of certain constructs—the sets at successive levels. Since the ordering of statements can be arranged so that the earlier ones will deal with the more inclusive sets, we can stop the process of setting up these statements at any convenient point, and accept the unfinished list of statements as an approximation to the distributional facts—knowing that the subsequent statements will only make subsidiary corrections to the earlier

[1] The investigation of historical regularity without direct regard to descriptive (synchronic) structure was the major achievement of the linguists of the late eighteen hundreds. There are incipient studies of historical-descriptive interrelations, as in H. M. Hoenigswald, Sound Change and Linguistic Structure, *Language* 22(1946). 138–43; cf. A. G. Juillald, A Bibliography of Diachronic Phonemics, *Word* 9(1953). 198–208. The independent study of descriptive structure was clarified largely by Ferdinand de Saussure's *Cours de linguistique générale*, the Prague Circle in its *Travaux du Cercle linguistique de Prague*, Edward Sapir in various writings, and Leonard Bloomfield's *Language*.

[2] These approaches are discussed by Martin Joos, Description of Language Design, *Journal of the Acoustical Society of America* 22(1950).702–8, and W. F. Twaddell, *ibid.* 24(1952).607–11.

statements. (This is not the case for the direct enumeration of restrictions, where the restrictions to be enumerated after a given point may be greater than those enumerated before.)

In view of this we may say that there is not only a body of facts about the relative occurrence of elements in a language, but also a structure of relative occurrence (i.e. of distribution). Hence the investigation of a language entails not only the empirical discovery of what are its irreducible elements and their relative occurrence, but also the mathematical search for a simple set of ordered statements that will express the empirical facts.[3] It may turn out that several systems of statements are equally adequate, for example several phonemic solutions for a particular language (or only, say, for the long vowels of a language). It may also be that different systems are simpler under different conditions. For example, one system may be adequate in terms of successive segments of sound (with at most stress and tone abstracted), while another system may be simpler if we admit the analysis of the sounds into simultaneous components of varying lengths. Or one system of stating distribution in respect to near neighbors (the usual environment for phonemic solutions) may be simple by itself, but if we are to imbed it in other statements about farther neighbors we may find that when we choose a modified system the statements covering the imbedding are simpler (i.e. a different phonemic solution may be more convenient for use in statements about morphemes). If the distributional structure is to be used as part of a description of speech, of linguistic behavior, then we will of course accept only such structures as retain a passably simple relation to the phonetic features. But for some other purpose, such as transmission or systemic analysis, phonetic complexity may be no serious objection. In any case, there is no harm in all this non-uniqueness,[4] since each system can be mapped onto the others, so long as any special conditions are explicit and measurable.

Various questions are raised by the fact that there can be more than one (non-trivial) structural statement for a given language. Can we say whether a particular item of structural analysis contributes to the simplicity of the system? It may be possible to do this: For example, if a given analysis involves a particular classification of elements (say, verbs), we may try some variation on this classification (say, by subdivision into transitive and intransitive—distributionally defined) and see whether the resulting analysis is simpler or not. Can we say what is invariant under all the possible distributional structures for a given body of data? For example, for all the phonemic solutions in a given language, there remains constant the minimal network of phonemically distinct utterance-pairs in terms of which we can distinguish every phonemically distinct utterance.

The various structural systems considered here all have this in common, that

[3] For a discussion of simplicity in this connection, see a forthcoming article by Noam Chomsky, Some Comments on Simplicity and the Form of Grammars.

[4] Y. R. Chao, The Non-uniqueness of Phonemic Solutions of Phonetic Systems, *Bulletin of the Institute of History and Philology, Academia Sinica* 4(1934).363–98. Cf. the two solutions of Annamese phonemes in M. B. Emeneau, *Studies in Vietnamese (Annamese) Grammar* 9–22.

they list items and their occurrences. There is at least one other type of structural statement which is essentially distributional but couched in different terms. This is the style which describes one linguistic form as being derived by some process (operation) from another. The item style says: Form *A* includes elements $e + f$ while form *B* includes elements $e + g$; and thus it describes all forms as combinations of elements. The process style says: Form *A* is derived from *B* by changing *f* into *g*; and thus it describes most forms as derived from certain base forms. The combinatorial or item style, which has a more algebraic form, is more parsimonious and representative for much of linguistic data. The process style, which is more similar to historical statements, is useful in certain situations, especially in compact morphophonemics.[5] Both styles are based solely on the relative occurence of parts, and are therefore distributional.

1.2. Reality of the structure.

Some question has been raised as to the reality of this structure. Does it really exist, or is it just a mathematical creation of the investigator's? Skirting the philosophical difficulties of this problem, we should in any case realize that there are two quite different questions here. One: Does the structure really exist in the language? The answer is yes, as much as any scientific structure really obtains in the data which it describes: the scientific structure states a network of relations, and these relations really hold in the data investigated.[5a]

Two: Does the structure really exist in the speakers? Here we are faced with a question of fact, which is not directly or fully investigated in the process of determining the distributional structure. Clearly, certain behaviors of the speakers indicate perception along the lines of the distributional structure: for example, the fact that while people imitate non-linguistic or foreign-language sounds, they "repeat" utterances of their own language[6] (i.e. they reproduce the utterance by substituting, for the sounds they heard, the particular corresponding variants which they habitually pronounce; hence the heard sounds are perceived as members of correspondence sets). There are also evidences of perception of sounds in terms of their morphophonemic memberships.[7]

A reasonable expectation is that the distributional structure should exist in the speakers in the sense of reflecting their speaking habits.[8] Indeed, responses

[5] This kind of formulation is best expressed in the work of Sapir and Newman; cf. reviews of *Selected Writings of Edward Sapir* (D. Mandelbaum, ed.), in *Language* 27(1951). 289–92; and of Stanley Newman, *Yokuts Language of California* in *International Journal of American Linguistics* 10(1944).196–211.

[5a] An opposition has sometimes been claimed between real facts and mathematical manipulation of structure. This claim ignores the fact that science is (among other things) a process of indicating much data by few general statements, and that mathematical methods are often useful in achieving this. Mathematical and other methods of arranging data are not a game but essential parts of the activity of science.

[6] As pointed out by Kurt Goldstein, *Language and language disturbances* 71, 103.

[7] E.g. in Edward Sapir, La réalité psychologique des phonèmes, *Journal de Psychologie Normale et Pathologique* 30(1933).247–65 (translated in David Mandelbaum, ed., *Selected Writings of Edward Sapir* 46–60).

[8] C. F. Hockett, review of *Recherches Structurales* in *International Journal of American Linguistics* 18(1952).98.

along the lines of distributional structure can be found in experimental psychology work.[9] However, different speakers differ in the details of distributional perception. One speaker may associate the stem of *nation* with that of *native*, while another may not: should the morpheme analysis be different for the two idiolects (individual dialects)? Even if we take the speaking habits to be some kind of social summation over the behaviors (and habits) of all the individuals, we may not find it possible to discover all these habits except by investigating the very speech events which we had hoped to correlate with the (independently discovered) habits.

If, as Hockett proposes, we measure the habits by the new utterances which had not been used in the structural description, we have indeed a possible and sensible measure; and this applies both to real productivity (the use of elements in environments in which they had not occurred before), and also to arbitrarily unused data (utterances which may have occurred before but which had not been used in deriving the distributional structure). However, even when our structure can predict new utterances, we do not know that it always reflects a previously existing neural association in the speakers (different from the associations which do not, at a given time, produce new utterances). For example, before the word *analyticity* came to be used (in modern logic) our data on English may have contained *analytic*, *synthetic*, *periodic*, *periodicity*, *simplicity*, etc. On this basis we would have made some statement about the distributional relation of *-ic* to *-ity*, and the new formation of *analyticity* may have conformed to this statement. But this means only that the pattern or the habit existed in the speakers at the time of the new formation, not necessarily before: the "habit"—the readiness to combine these elements productively—may have developed only when the need arose, by association of words that were partially similar as to composition and environment.

For the position of the speakers is after all similar to that of the linguist. They have heard (and used) a great many utterances among which they perceive partial similarities: parts which occur in various combinations with each other. They produce new combinations of these along the lines of the ones they have heard. The formation of new utterances in the language is therefore based on the distributional relations—as changeably perceived by the speakers—among the parts of the previously heard utterances.[10]

Concerning any habit, i.e. any predisposition to form new combinations along particular distributional lines rather than others, we know about its existence in the speakers only if we have some outside evidence (such as association tests), or if new formations of the type in question have been formed by these speakers. The frequency of slips, new formations, etc., is enough to make us feel that the bulk of the major structural features are indeed reflected in speaking habits—habits which are presumably based, like the linguist's analysis, on the distribu-

[9] As pointed out to the writer by A. W. Holt.

[10] This applies to the grammatical innovation involved in new formations; the selection of morphemes within a class is determined, not only by these "grammatical" associations but also semantically. Cf. the first paragraph of §1.1 above.

tional facts. Aside from this, all we know about any particular language habit is the probability that new formations will be along certain distributional lines rather than others, and this is no more than testing the success of our distributional structure in predicting new data or formations. The particular distributional structure which best predicts new formations will be of greatest interest from many (not all) points of view; but this is not the same as saying that all of that structure exists in the speakers at any particular time prior to the new formations.[11]

2. Distribution and meaning.

2.1. Is there a parallel "meaning structure"?

While the distinction between descriptive (synchronic) structure and historical change is by now well known, the distinction between distributional structure and meaning is not yet always clear. Meaning is not a unique property of language, but a general characteristic of human activity. It is true that language has a special relation to meaning, both in the sense of the classification of aspects of experience, and in the sense of communication. But the relation is not simple. For example, we can compare the structures of languages with the structure of the physical world (e.g. the kind of phenomena that are expressed by differentiation and integration in calculus), or with what we know about the structure of human response (e.g. association, transference). In either case, it would be clear that the structure of one language or another does not conform in many respects to the structure of physical nature or of human response—i.e. to the structure of objective experience from which we presumably draw our meanings. And if we consider the individual aspects of experience, the way a person's store of meanings grows and changes through the years while his language remains fairly constant, or the way a person can have an idea or a feeling which he cannot readily express in the language available to him, we see that the structure of language does not necessarily conform to the structure of subjective experience, of the subjective world of meanings.[12]

All this is not to say that there is not a great interconnection between language and meaning, in whatever sense it may be possible to use this word. But it is not

[11] Here we have discussed whether the distributional structure exists in the speakers as a parallel system of habits of speaking and of productivity. This is quite different from the dubious suggestion made at various times that the categories of language determine the speakers' categories of perception, a suggestion which may be a bit of occupational imperialism for linguistics, and which is not seriously testable as long as we have so little knowledge about people's categories of perception. Cf. for the suggestion, Benjamin L. Whorf, The Relation of Habitual Thought and Behavior to Language, *Language, Culture and Personality (Sapir Memorial Volume*, A. I. Hallowell, L. Spier, S. Newman, eds.) 75–93; Languages and Logic, *The Technology Review* 43–6 (1941); and against it, Eric H. Lennenberg, Cognition in Ethnolinguistics, *Language* 29(1953).463–71; Lewis S. Feuer, Sociological Aspects of the Relation Between Language and Philosophy, *Philosophy of Science* 20(1953).85–100.

[12] In E. G. Schachtel's On Memory and Childhood Amnesia, *Psychiatry* 10(1947).1–26 it is suggested that the experiences of infancy are not recallable in later life because the selection of aspects of experience and the classification of experience embodied in language, which fixes experience for recall, differs from the way events and observations are experienced (and categorized) by the infant.

a one-to-one relation between morphological structure and anything else. There is not even a one-to-one relation between the vocabulary and any independent classification of meaning: We cannot say that each morpheme or word has a single or central meaning, or even that it has a continuous or coherent range of meanings. Accidents of sound change, homonymity, borrowing, forgotten metaphors, and the like can give diverse meanings to a number of phonemic occurrences which we have to consider as occurrences of the same morpheme. Aside from this, if we consider the suggestion of Kurt Goldstein[13] that there are two separate uses and meanings of language—the concrete (e.g. by certain brain-injured patients) and the abstract—it would follow that the same grammatical structure and much the same vocabulary can carry quite different types of speaking activity.

The correlation between language and meaning is much greater when we consider connected discourse. To the extent that formal (distributional) structure can be discovered in discourse, it correlates in some way with the substance of what is being said; this is especially evident in stylized scientific discourse (e.g. reports on experimental work) and above all in the formal discourses (proofs) of mathematics and logic. However, this is not the same thing as saying that the distributional structure of language (phonology, morphology, and at most a small amount of discourse structure) conforms in some one-to-one way with some independently discoverable structure of meaning. If one wishes to speak of language as existing in some sense on two planes—of form and of meaning—we can at least say that the structures of the two are not identical, though they will be found similar in various respects.

2.2. Are morphemes determined by meaning?

Since there is no independently-known structure of meanings which exactly parallels linguistic structure, we cannot mix distributional investigations with occasional assists from meaning whenever the going is hard. For example, if the morphemic composition of a word is not easily determined, we cannot decide the matter by seeing what are the component meanings of the word and assigning one morpheme to each: Do *persist, person* contain one morpheme each or two? In terms of meaning it would be difficult to decide, and the decision would not necessarily fit into any resulting structure. In terms of distribution we have *consist, resist, pertain, contain, retain,* etc. (related in phonemic composition and in sentence environment), but no such set for *person;* hence we take *persist* as two morphemes, *person* as one.

Although rough indications of meaning are often used heuristically to guess at the morphemes of a word or utterance, the decision as to morphemic composition is always based on a check of what sections of that word or utterance are substitutable in a structured (patterned) way in that environment; as roughly indicated in the example above.

Where the meanings (in most cases, the translations) are not immediately suggestive, the analysis is laboriously distributional without any heuristic aids to

[13] *Human Nature in the Light of Psychopathology:* The William James Lectures for 1938–39, ch. 3.

test. For example in the Cherokee verb prefixes, we find scores of forms,[14] e.g. /agwalənə́ʔəgi/ 'I started', /sdəgadhénoha/ 'I and another are searching for you', /sdəgadhénohəgi/ 'I searched for you two'. These have obviously personal reference, but it is impossible to separate out a small set of phonemic segments which will mean 'I' or 'I as subject', 'I as object', etc. It is nevertheless possible to discover the morphemes distributionally. First we identify the words by their distributional relation to the rest of the sentence. We find that certain words with many different stems and a few different prefixes have certain types of environment in common. For example /zinəgaliʔa/ 'I am cleaning' and /agiyoseha/ 'I am hungry' occur in certain environments in which /uniyoseha/ 'they are hungry' does not occur. We take a set of words each with different stems but which have the same environment in the sense referred to above. We will assume that the sameness in this feature of the environment correlates with some morphemic part that is the same in all these words (and is obviously not the stem).[15] This means that the different prefixes of these words contain alternants of the same morpheme; and we try to state a morphophonemic relation between /z/, /(a)g/, etc., giving the environing conditions (in phonemic rather than morphemic terms if possible) in which each alternant occurs: we write the morpheme {z} and translate it 'I'. Another set, containing e.g. /ozinəgaliʔa/ 'I and others are cleaning', /ogiyoseha/ 'I and others are hungry', would thus be analyzed (in the same manner, but with the aid of {z}) as containing two morphemes, {o} 'others' and {z} 'I'. If we now turn to the set containing /osdinəgaliʔa/ 'I and another are cleaning', /oginiyoseha/ 'I and another are hungry', etc., our morphophonemic knowledge about {z} enables us to separate out /d/, /n/ etc. as alternants of some third morpheme {n}, with undetermined meaning. In /iginiyoseha/ 'you and I are hungry' our known morphophonemics enables us to analyze the prefix as an alternant of {z} plus an alternant of this same {n}, where it seems to have the meaning 'you'. However, in /hinəgaliʔa/ 'you (sg.) are cleaning' we are unable to fit the /h/ into the morphophonemic regularities of {n}, and thus set up a new morpheme {h} 'you'; and in /sdinəgaliʔa/ 'you two are cleaning' we can satisfy the morphophonemic regularities by saying that there are two morphemes: the /s/ alternant of {h} plus the /d/ alternant of {n}.

In this way we can divide each prefix into a unique combination of morphophonemic alternants of the following morphemes: {z} 'I', {h} 'you (sg.)', {a} 'third person sg.', {i} 'plural' (always including 'you', at least due to absence of {o}), {o} roughly 'person(s) excluding you', {n} roughly 'another person, you as first choice'. These morphemes were obtained as solutions to the environmental

[14] The following analysis can be fully understood only if one checks through the actual lists of Cherokee forms. The few forms cited here are taken from William D. Reyburn, Cherokee Verb Morphology II, *International Journal of American Linguistics* 19(1953). 259–73. For the analysis, see the charts and comments in Reyburn's work and in Z. S. Harris, Cherokee Skeletal Grammar, and Cherokee Grammatical Word Lists and Utterances, in the Franz Boas Collection of the American Philosophical Society Library.

[15] This assumption is based on the fact that each morpheme has a different distribution (§2.36), so that same feature of environment points to the same morpheme.

regularities of the prefixed phonemes. The translations offered above are an attempt to assign a single meaning to each on the basis of the meanings of all those words in which it occurs. If we write the prefixes morphophonemically, then the meanings of some of the occurring combinations are: {ozn} (phonemically /osd/ etc.) 'I and he', {oz} 'I and they', {zn} 'I and you (sg.)' {iz} 'I, you, and they', {h} 'you (sg.)', {hn} 'you two', {in} 'you (pl.)'. From this we can try to extract (as above) a single meaning contribution which {n} or {o} or {i} bring to each combination in which they are included. But it was not the isolation of these complicated central meanings (if that is always non-trivially possible) that led us to recognize {n} etc. as morphemes. We do not even know that these central meanings exist for the speakers: the speakers may be subjectively using two homonymous {n} morphemes, or they may be using these prefix combinations as fixed whole entities with only a vague impression of the phonemic and morphophonemic regularities.[16]

So far, we have not touched the great majority of verb forms, those which have objects together with the subjects. By using the morphophonemic relations established previously, we are able to extract the morphemes above from some of these new combinations, and small extensions of the morphophonemics reveal these morphemes in yet other combinations. Then we analyze the prefix in /gəiha/ 'I am killing you' as {z} + {n}, and in /sgwúsədohda/ 'you covered me' as {h} + {z}; and certain order statements about the two prefix components indicate the subject-object relation. The remaining phonemes of some of these prefixes can be grouped by rather simple morphophonemics into a few additional morphemes like {g} 'animate object'; and so we finally obtain a morphemic analysis of all the prefixes. This analysis does not necessarily correlate with any meaning units we may have in mind about person and number. For example, it gives the same morphemes {znn} for the prefix in /sdəgadhénoha/ 'I and another are searching for you (whether sg. or dual but not plural)' and in /sdəgadhénohəgi/ 'I searched for you two'. Even if we find different phonemes with different meanings, e.g. /izə-gow'diha/ 'I and he see you (pl.)' and /izəy-olighi/ 'I and they know you (sg.)' the analysis may say that these are alternants of the same morphemic composition {izn}; in that case both meanings can be obtained for each form.

The methods indicated so sketchily above suggest how the morphemic composition of a word or utterance can be determined by the occurrence of each phoneme sequence relative to others: e.g. *per, con* relative to *sist, tain;* or /z/ /gi/, /o/, etc. relative to various features of environment which are common to /z/ and /gi/ as against /o/. The final decision as to morphemic analysis always depends on this relative occurence of phoneme sequences, since the grammar then proceeds to state compactly the relative occurrence of the morphemes. That is, we set up as morphemes those phonemic sequences (or features) such that all utterances are compactly statable relative occurrences of them.

The chief difficulty with this is that it provides us only with a criterion that

[16] Since new formations of these combinations do not appear, we cannot apply the productivity tests of §2.1 to discover the speakers' morphemic recognition.

tells us whether a given phoneme sequence is a morpheme or not; more exactly, whether a particular segmentation of an utterance (once we propose it) divides it into morphemic segments. It does not provide us with a procedure which will directly yield a morphemic segmentation of an utterance. There is available, however, a procedure which yields most if not all of the morphemic segmentations of an utterance. In outline it is as follows: Given any test utterance, associate many utterances whose first phoneme is the same as that of the test utterance; and note how many different phonemes follow the first in these utterances. Then consider utterances whose first two phonemes are the same as the first two of the test utterance, and note how many different phonemes follow the first two in these. And so on. If after the first n phonemes the number of different phonemes which follow the nth (in the associated utterances) is greater than the number after the first $n-1$ phonemes or the first $n+1$, then we place a tentative morpheme boundary after the nth. Various operations are needed to correct and check the correctness of each result; but together with the final test of patterned relative occurrence, this yields the morphemes of a language without any reference to meaning or informant response.

2.3. Meaning as a function of distribution.

Distribution suffices to determine the phonemes and morphemes, and to state a grammar in terms of them. However, both (a) in determining the elements and (b) in stating the relations between them, it turns out that the distributional structure does not give ideal coverage. It must either leave many details unsaid, or else become extremely complicated. For example: (a) Morphemes are determined on the basis of a patterned independence (replaceability in utterances) in respect to other morphemes (or phoneme sequences); but not all morphemes have the same degree of independence: compare *hood* (*boyhood*) with *ness* (*bigness*). (b) The grammatical statements group morphemes into classes, and then say that certain sequences of these classes occur; but not every member of the one class occurs (in any actual body of data) with every member of the other: not every adjective occurs with every noun. Finally we may mention one other respect in which distribution fails to cover all the facts about speech occurrence: (c) We can state distributional regularities only within narrow domains—for phonology usually the immediately neighboring phonemes, for morphology usually the sentence or some part of the sentence.

At all these points where simple distributional regularities are no longer discoverable, people often revert to the position of our man in the street (§1.1) and say that here the only determinant is meaning: (a) *hood* has a meaning which ties it to certain few nouns; (b) with a given noun, e.g. *doctor*, there will be used those adjectives that make sense with it; (c) beyond the sentence there are no significant formal restrictions on what one says, and sentences are strung along purely according to meaning. Now meaning is of course a determinant in these and in other choices that we make when we speak. But as we make these choices we build a stock of utterances each of which is a particular combination of particular elements. And this stock of combinations of elements becomes a factor in the way later choices are made (in the sense indicated in the last two para-

graphs of §1.2); for language is not merely a bag of words but a tool with particular properties which have been fashioned in the course of its use. The linguist's work is precisely to discover these properties, whether for descriptive analysis or for the synthesis of quasi-linguistic system. As Leonard Bloomfield pointed out, it frequently happens that when we do not rest with the explanation that something is due to meaning, we discover that it has a formal regularity or "explanation." It may still be "due to meaning" in one sense, but it accords with a distributional regularity.

If we investigate in this light the areas where there are no simple distributional regularities, we will often find interesting distributional relations, relations which tell us something about the occurrence of elements and which correlate with some aspect of meaning. In certain important cases it will even prove possible to state certain aspects of meaning as functions of measurable distributional relations.

(a) There are different degrees of independence (§3.3). We find complete dependence in the various phonemes of one morpheme, or in the various parts of a discontinuous morpheme (including grammatical agreement). In *hood* we have sufficient independence to make it a separate morpheme, but it is limited to very few predecessors. In *ness* there is more independence. The degree of independence of a morpheme is a distributional measure of the number of different morphemes with which it occurs, and of the degree to which they are spread out over various classes or subclasses. The various members of a distributional class or subclass have some element of meaning in common, which is stronger the more distributional characteristics the class has. The major classes have the kind of common meanings that are associated, say, with the words "noun" or "adjective."

(b) The fact that, for example, not every adjective occurs with every noun can be used as a measure of meaning difference. For it is not merely that different members of the one class have different selections of members of the other class with which they are actually found. More than that: if we consider words or morphemes A and B to be more different in meaning than A and C, then we will often find that the distributions of A and B are more different than the distributions of A and C. In other words, difference of meaning correlates with difference of distribution.

If we consider *oculist* and *eye-doctor*[17] we find that, as our corpus of actually-occurring utterances grows, these two occur in almost the same environments, except for such sentences as *An oculist is just an eye-doctor under a fancier name*, or *I told him Burns was an oculist, but since he didn't know the professional titles, he didn't realize that he could go to him to have his eyes examined.* If we ask informants for any words that may occupy the same place as *oculist* in sentences like the above (i.e. have these same environments), we will not in general obtain *eye-doctor;* but in almost any other sentence we would. In contrast, there are many sentence environments in which *oculist* occurs but *lawyer* does not: e.g. *I've had my eyes examined by the same oculist for twenty years*, or *Oculists often have their prescription blanks printed for them by opticians.* It is not a question of

[17] This particular pair was suggested to me by Y. Bar-Hillel, who however considers that distributional correlates of meaning differences cannot be established.

whether the above sentence with *lawyer* substituted is true or not; it might be true in some situation. It is rather a question of the relative frequency of such environments with *oculist* and with *lawyer*, or of whether we will obtain *lawyer* here if we ask an informant to substitute any words he wishes for *oculist* (not asking what words have the same meaning). These and similar tests all measure the probability of particular environments occurring with particular elements, i.e. they measure the selections of each element.

It is impossible to obtain more than a rough approximation of the relatively common selection of a given word (with almost no indication of its rarer selection). But it is possible to measure how similar are the selection approximations of any two words (within various sets of data). If for two elements A and B we obtain almost the same list of particular environments (selection), except that the environment of A always contains some X which never occurs in the environment of B, we say that A and B are (complementary) alternants of each other: e.g. *knife* and *knive-*. If A and B have identical environments throughout (in terms of our data tests) we say that they are free variants: e.g. perhaps for /ekənamiks/ and /iykənamiks/ *economics*. If the environments of A are always different in some regular way from the environments of B, we state some relation between A and B depending on this regular type of difference: e.g. *ain't* and *am not* have frequent differences of a certain type in their environments (*ain't goin'* but *am not going*) which we would call dialectal. If A and B have almost identical environments except chiefly for sentences which contain both, we say they are synonyms: *oculist* and *eye-doctor*. If A and B have some environments in common and some not (e.g. *oculist* and *lawyer*) we say that they have different meanings, the amount of meaning difference corresponding roughly to the amount of difference in their environments. (This latter amount would depend on the numerical relation of different to same environments, with more weighting being given to differences of selectional subclasses.) If A and B never have the same environment, we say that they are members of two different grammatical classes (this aside from homonymity and from any stated position where both these classes can occur).

While much more has to be said in order to establish constructional methods for such a classification as above, these remarks may suffice to show how it is possible to use the detailed distributional facts about each morpheme. Though we cannot list all the co-occurrents (selection) of a particular morpheme, or define its meaning fully on the basis of these, we can measure roughly the difference in selection between elements, say something about their difference in meaning, and also (above and §4.1) derive certain structural information.

(c) If we investigate the relative occurrence of any part of one sentence in respect to any part of the neighboring sentences in the same discourse, we will find that there are certain regularities (§3.5 end). The sequence of sentences is not entirely arbitrary; there are even certain elements (e.g. pronouns) whose occurrence (and meaning) is specifically related to the grammatically-restricted occurrence of certain other morphemes in the neighboring sentences (§4.1, first paragraph). Such regularities (and meanings) will not extend from one discourse

to another (except to another related in some relevant way to the first, e.g. successive lectures of a series). A consecutive (or seriate) discourse of one or more persons is thus the fullest environmental unit for distributional investigation.[18]

3. Distributional analysis.

We now review briefly the basic analysis applicable to distributional facts.

3.1. Element. The first distributional fact is that it is possible to divide (to segment) any flow of speech into parts, in such a way that we can find some regularities in the occurrence of one part relative to others in the flow of speech. These parts are the discrete elements which have a certain distribution (set of relative locations) in the flow of speech; and each bit of speech is a particular combination of elements. The first operation is purely segmenting, arbitrary if need be. The first step of segmenting has to be independent of any particular distributional criterion, since we cannot speak of distributional relations until we have not only segments but also a similarity grouping of them (§3.2). After the first segmenting of utterances, each segment is unique and has a unique environment (completely different from every other one); after the segments have been compared, and "similar" ones grouped together, we find that various of these similarity groupings have partially similar and partially different environments. Hence we can speak about the distributional relations of these similarity groupings.

If we wish to be able, in the later operations (§3.3–4), to obtain elements (or classes of elements) whose distributions will have maximum regularity, we have to divide not only the time flow into successive portions, but also any single time segment (or succession of time segments) into simultaneous components (of one segment length, e.g. a tone, or longer, e.g. a pitch-stress contour). After we have set up the phonetically more obvious segmentations and simultaneities, and have studied their distribution, we may find that more regular distributions can be obtained if we change our original segmentation of elements, even to ones that are phonetically less obvious, and even if some of our adjusted elements become components which extend over various numbers of other elements.

3.2. Similarity. Another essential distributional fact is that some elements are similar to others in terms of certain tests; or are similar in the sense that if we group these similar elements into sets ("similarity groupings"), the distribution of all members of a set (in respect to other sets) will be the same as far as we can discover. This reduces ultimately to the similarity of sound segments under repetition, or in the pair test: x_1 is similar to x_2 but not to y_1 if, when one native

[18] It should be clear that only after we discover what kinds of distributional regularities there are among successive elements or sections in discourses can we attempt any organized semantic interpretation of the successions discovered. Various types of discourses have various types of succession (of sentences, clauses, or other intervals). In mathematics and the constructed "languages" of logic, certain conditions are imposed on what sentences can appear in succession in their connected discourses (proofs): each sentence (line in a proof) has to be a theorem or else derived from a preceding sentence in a particular way. This situation does not hold for natural languages, where the truth-value of logic is not kept constant through successive sentences, and where the types of succession are more varied.

speaker repeats x_1z, x_2z, y_1z, . . ., a second speaker can guess correctly whether x_1z as against y_1z is being said, but not whether x_1z as against x_2z is being said. We call x_1 and x_2 free variants of each other (or members of a similarity grouping). Note that the pair test involves discrimination of sound but not of meaning.

3.3. Dependence (serial). To obtain a least set of elements sufficient for description we join any elements which are completely dependent: if A is a set of similar elements (a similarity grouping) and so is B, and (in a particular type of environment) only AB occurs (not necessarily contiguously), never A or B alone, then we set up AB as a single element (a single set of similar elements).

Thereafter we don't have any two elements which are completely dependent upon each other in occurrence. But our elements have various degrees of dependence: for each element we can say that any utterance (or shorter domain) which contains it will also contain such and such other classes. For example, morpheme A may occur always close to (i.e. within a stateable distance from) any one of a few or many B_1, B_2, . . . If the sequence B_1A occurs in environments X, it may be that B_1 by itself also occurs in X (e.g. *kingdom* and *king*), or that B_1 does not (e.g. *kingly* and *king*). The B_1 with which A occurs may all have the same types of environment when they occur without A (e.g. all predecessors of *dom* are nouns), or some may have one type and some another (e.g. *ish* occurs with both nouns and adjectives). These are a few of the various degrees and types of occurrence-dependence which an element can have to the elements that occur in the same utterances as it does.

3.4. Substitutability (parallel). It will in general appear that various elements have identical types of occurrence-dependence. We group A and B into a substitution set whenever A and B each have the same (or partially same) environments X (X being at first elements, later substitution sets of elements) within a statable domain of the flow of speech. This enables us to speak of the occurrence-dependence of a whole set of elements in respect to other such sets of elements. Some of the types of partial sameness of environment were listed in §2.3(b).

The elements of distributional structure are usually obtained by the operations of §3.1, §3.2 and the first paragraph of §3.3. The distributional relations are usually combinations of §3.3 and §3.4. For example, *hood* occurs after few morphemes N_1, N_2, . . . of a certain substitution set ("nouns"), *ish* after many of them, *s* and its alternants after all or almost all of them. N_i + *hood* or N_i + *s* occur in the same large environments in which N_i occur alone. But N_i + *ish* occur in different environments than N_i alone; however *ish* also occurs after many members of another substitution set, A_1, A_2, . . . ("adjectives"), and both N_i + *ish* and A_i + *ish* occur in the larger environments of A_i alone.

3.5. Domains. All the statements about dependence and substitutability apply within some specified domain, the domain being determined either by nature (e.g. silence before and after an utterance) or by the types of environment within which there is regularity (e.g. the narrow restriction of *hood* is only to what precedes it, and only to the first morpheme in that direction). It is often possible to state the co-occurrences of elements within a domain in such a way that that domain then becomes the element whose co-occurrences are regular within a larger

domain: e.g. the occurrences of stems and suffixes within word-length, and of words within phrases. Common types of domain are the word, phrase, clause. In many cases the stretches of speech covered by certain long pitch and stress components (or fixed sequences of short pitch and stress components) are identical with the domains of distributional relations: word, sentence.

Although grammar has generally stopped with the sentence, it is possible to find distributional regularities in larger domains. There are certain sentence sequences in which the second can be described as a fixed modification of the first (e.g., with certain restrictions, in the case of questions and answers in English). There are certain types of distributional relation (e.g. between English active and passive, between *buy* and *sell*) which have particular kinds of regularity in (not necessarily immediately) neighboring sentences. For example, if one sentence contains noun A + active (transitive) verb B + noun C, and a neighboring sentence contains C + verb + A, there is a certain likelihood that the verb will be the passive of B; or if the neighboring sentence contains C + the passive of B + some noun, there is a certain likelihood that the second noun will be A or some noun which elsewhere in that discourse has similar individual environments (selection) to those of A. And if one sentence contains A *buys* B *from* C, and a neighboring sentence contains C *sells* B *to* + some noun, there is a good likelihood that the noun will be A or an environmentally similar noun (and given C + some verb + B *to* A, we may expect the verb to be *sell* or some environmentally similar one).[19]

Finally, if we take a whole connected discourse as environment, we find that there are certain substitution sets of morphemes which occur regularly (relative to the other sets) throughout the discourse or some portion of it;[20] these are not the major substitution sets of the language (e.g. nouns) or its grammatical subclasses, but new groupings which are often relevant only to that one discourse. And there are certain sequences of these sets which constitute the subdomains of the discourse, i.e. such that the sets are regular within these intervals and the intervals are regular within the discourse; these intervals are not necessarily sentences or clauses in the sense of grammatical structure. The regularities in a discourse are far weaker and less interrelated than those within a sentence; but they show that occurrence-dependence (and the environment relevant for distribution) can extend throughout a whole discourse.

3.6. Data. The distributional investigations sketched above are carried out by

[19] Such relations as that of active to passive, or *buy* to *sell*, are essentially substitutability relations (§3.4), i.e. they show that certain elements have similar environments (e.g. partially inverted ones). The fact that they may appear in neighboring sentences is a serial relation (§3.3) which is a secondary characteristic of certain substitutabilities. Relations like that of active to passive are different from the essentially serial relations of successive intervals of a discourse, discussed at the end of §3.5.

[20] The fact that a discourse contains several or many occurrences of a given substitution class, often in parallel positions, brings out a rare relation in linguistics: the order of occurrence of various members of the same class. Something like this comes up in compound nouns, or in successions of two or more adjectives (sometimes with preferred order). Usually, if two members of a class occur in one domain, their order is not regular (e.g. in most cases of N *and* N); but in compound nouns, for instance, certain members are frequent in the first N position, and others in the second.

recording utterances (as stretches of changing sound) and comparing them for partial similarities. We do not ask a speaker whether his language contains certain elements or whether they have certain dependences or substitutabilities. Even though his "speaking habits" (§1.2) yield regular utterances, they are not sufficiently close to all the distributional details, nor is the speaker sufficiently aware of them. Hence we cannot directly investigate the rules of "the language" via some system of habits or some neurological machine that generates all the utterances of the language. We have to investigate some actual corpus of utterances, and derive therefrom such regularities as would have generated these utterances—and would presumably generate other utterances of the language than the ones in our corpus. Statements about distribution are always made on the basis of a corpus of occurring utterances; one hopes that these statements will also apply to other utterances which may occur naturally. Thus when we say that the selectional difference in *oculist/lawyer* is greater than in *oculist/eye-doctor* (§2.3), or that the selection of nouns around the passive verb is the same as the selection around the active verb but with inverted order (§4.1) we mean that these relations will be approximated in any sufficiently large corpus (especially one built with the aid of eliciting), and that they will presumably apply to any sufficiently large additions to the corpus.

In much linguistic work we require for comparison various utterances which occur so infrequently that searching for them in an arbitrary corpus is prohibitively laborious. To get around this, we can use various techniques of eliciting, i.e. techniques which favor the appearance of utterances relevant to the feature we are investigating (without influencing the speaker in any manner that might bring out utterances which would not have sometimes occurred naturally). In particular, investigations of the selections of particular morphemes (§2.3, 4.1) can hardly be carried out without the aid of eliciting. Eliciting is a method of testing whether a certain utterance (which is relevant to our investigation) would occur naturally: in effect, we try to provide a speaker with an environment in which he could say that utterance—if he ever would naturally say it—without extracting it from him if he wouldn't. For example, if we are testing the active/passive relation we might offer a speaker noun A_1 + transitive verb B_1 and ask him to complete the sentence in many ways, obtaining a particular selection C_1, C_2, . . . after the verb. Then we can offer a speaker the passive verb B_1 + A_1 and ask him to begin the sentence in many ways, checking whether we get about the same selection C_1, C_2, . . . before the verb. We can repeat this for various A_i, and then for various B_i.

4. Distributional relations.

The methods of §3 yield first of all a representation of each utterance as a combination of elements. They also yield a set of statements about the utterances: what elements and regularities of combination suffice to represent the utterances. One can go beyond this and study the kinds of regularities, and the kinds of relations among elements. As was pointed out at the end of §2.3(b), certain correlations may be discovered even in those distributional facts which are too individual to be directly useful.

4.1. As an example of the latter we may consider selectional similarity. For

instance, it is impossible to list all the verbs that follow each particular noun, or all the verbs that follow *who*. But it is possible to state the following relation between the verb selection of nouns and the verb selection of *who:* Under an eliciting test as in §3.6, we will get after *The pianist*— much the same verbs as we will get after *The pianist who*—, and so for every noun. This means that the verb selection of *who* is the same as the verb selection of the noun preceding *who*. We have here a distributional characteristic that distinguishes such pronominal elements from ordinary nouns.

Or we may consider the active/passive relation mentioned in §3.6. If we take a large number of sentences containing a transitive verb in English, e.g. *The kids broke that window last week*, we can elicit sentences consisting of the same verb but with the passive morpheme, the same nouns before and after it but in reverse order, and the same remainder of the sentence, e.g. *That window was broken by the kids last week*. Some of these sentences may be stylistically clumsy, so that they would not occur unless some special circumlocution were involved; but they are obtainable by otherwise valid eliciting techniques.[21] In contrast, if we seek such inversion without the passive, we will fail to elicit many sentences: we can get *The kids saw Mary last week* and *Mary saw the kids last week;* but to *The kids saw the movie* we will never—or hardly ever—get *The movie saw the kids* (even though this sentence is grammatical). Or if we seek such selectional similarity (with or without inversion) for *broke/will break* or the like, we will find the same selection as to preceding and following nouns, but not always as to the rest of the sentence: *The kids broke that window* and *The kids will break that window*, but not *The kids will break that window last week* or *The kids broke that window if they don't watch out*. It thus appears that, using only distributional information about an ordinarily elicited corpus, we can find a relation between the active verb and the passive verb which is different from the relation between *-ed* and *will*.

4.2. The distributional regularities can themselves be a subject of study. One can consider recurrent types of dependence and substitutabilities that are found in a language (or in many languages), and find on one level such relations as "subject" and "object" (semantic names for distributional positions), and on a higher level of generality such relations as "constituent" and "head of a construction" (if A occurs in environment X, and AB does too, but B does not, then A is the head of AB). One can consider the parts of a grammar which permit alternative distributional analyses, and check their relation to language change and dialect or idiolect interrelations (since probably every linguistic structure has some points which are structurally in flux). One can investigate what are the structural characteristics of those parts of a language which are productive. Furthermore, one can survey what is similar and what is different in a great many language structures, and how linguistic systems in general differ from such partially similar systems as mathematics and logistic "languages," sign languages, gestures, codes, music.

[21] There will be a few exceptions where the passive is not obtainable. And if we try to elicit the active on the basis of the passive, we run into the difficulty of distinguishing between *by* of the passive (*The letter was finished by Carl*) and *by* as preposition (*The letter was finished by noon*).

LA STRATIFICATION DU LANGAGE

LOUIS HJELMSLEV

On ne saurait rendre compte, même d'une façon rudimentaire, de la linguistique d'aujourd'hui — ni même, d'une façon plus générale, de la science de l'homme, dont elle fait partie — sans donner une large part à la double distinction entre *forme* et *substance* et entre *contenu* (*signifié*) et *expression* (*signifiant*). Cette double distinction en effet, introduite par F. de Saussure et développée dans certaines branches de la linguistique moderne, constitue le noyau autour duquel gravitent forcément, à des distances diverses, toutes discussions de méthode et de principe. L'introduction de cette double distinction a été une découverte, sinon (comme nous le croyons d'ailleurs pour notre part) d'une méthode nécessaire, du moins d'une méthode possible. Or, cette double distinction possible une fois découverte, on n'y échappe plus, et quelle que soit l'attitude qu'il adopte par rapport à ce problème ou aux multiples problèmes qui en dérivent, le linguiste est obligé de prendre conscience du problème fondamental soulevé par cette double distinction. Toute méthode linguistique, explicite ou non, peut et doit se définir par rapport aux deux distinctions fondamentales.

Pour notre part nous avons pensé faire œuvre utile en tirant expérimentalement les conséquences extrêmes de la double distinction saussurienne, en vue de permettre à la linguistique de faire ressortir nettement les avantages et les difficultés comportés par une telle axiomatique. Puisque une des définitions possibles (et même, selon nous, la définition la plus fondamentale) d'une *langue*, dans l'acception saussurienne de ce terme, est celle qui consiste à la définir comme une forme spécifique organisée entre deux substances: celle du contenu et celle de l'expression, donc comme une forme spécifique de contenu et d'expression, la tâche qui consiste à tirer toutes les conséquences de la double distinction mentionnée peut être ramenée à une formule encore plus simple: il s'agit en effet simplement de déduire toutes les conclusions qu'on peut dégager de la phrase finale du *Cours de linguistique générale:* «la linguistique a pour unique et véritable objet la langue envisagée en elle-même et pour elle-même». (Naturellement, *objet* doit être entendu ici dans le sens que lui attribue Pascal[1].) C'est dans ce sens que l'on peut qualifier la méthode ici préconisée comme étant celle de la linguistique *immanente*.

Qu'il nous soit permis de rappeler à ce propos une formule que nous avons présentée dès le début de notre activité dans ce domaine[2]:

«D'une façon générale, tous les auteurs qui ont traité de grammaire . . . posent le problème grammatical d'autant de points de vue différents. Par là même, ils ont contribué

[1] Cf. A. Lalande, *Vocabulaire de la philosophie*, 4e éd., p. 531, col. 2, alinéa B («ce que nous nous proposons d'atteindre ou de réaliser en agissant»).

[2] L'auteur, *Principes de grammaire générale*, 1928, p. 5.—C'est en 1931 que, en donnant notre contribution à la première inauguration du travail du Cercle linguistique de Copenhague, nous avons présenté la formule explicite de la «méthode immanente en linguistique» (cf. *Bulletin du Cercle ling. de Copenhague* II, p. 14).

largement à éclaircir le problème méthodologique. . . . Nous espérons cependant» (pouvoir) «compléter utilement la série des points de vue possibles . . . en y ajoutant un nouveau point de vue, jusqu'ici négligé, et qui pourtant nous semble être le point de vue principal, celui qui consiste à se placer, d'une manière empirique, sur le terrain du langage même, en délimitant le plus nettement possible les faits linguistiques, d'un côté, des faits non-linguistiques, de l'autre.»

En effet, cette attitude seule permettra de dégager, dans le réseau de fonctions dans lequel elle est ancrée, la langue comme l'objet auquel on vise et dont on part pour opérer toutes déductions.

Or les efforts pour creuser assez profondément nos assises sur cette base nouvelle se sont trouvés constituer un travail de bien longue haleine. Ce n'est que graduellement et par tâtonnements que nous sommes arrivé, si nous osons le dire, à mesurer toute la portée de la découverte, à découvrir toutes les conséquences qui en découlent, et à pousser à la perfection l'instrument fourni par les nouvelles notions. C'est ainsi que nous sommes arrivé à établir la doctrine connue sous le nom de *glossématique* qui, de fait et pratiquement, peut être caractérisée par quatre traits particuliers: 1° celui de recommander comme la seule adéquate une procédure analytique (dite aussi déductive, d'un terme qui s'est montré prêter à l'équivoque), et de considérer la synthèse (ou description des unités par les parties qui les composent, ou, pour mieux dire, par leurs fonctions intérieures génératrices) comme présupposant l'analyse; 2° celui d'insister sur la forme, jusqu'ici négligée en faveur de la substance; 3° celui de vouloir comprendre dans la forme linguistique celle du contenu, et non seulement celle de l'expression; et, en conséquence de ces principes, 4° celui de considérer le langage, dans le sens communément adopté par les linguistes, comme un cas particulier d'un système sémiotique, c'est-à-dire d'un système comportant des plans différents et, à l'intérieur de chaque plan, une différence entre forme et substance (réserve faite de l'absence de substance telle qu'on la constate dans le cas d'un système construit, p. ex. en linguistique génétique ou dans un calcul typologique, à moins qu'on n'y ajoute une manifestation spécifique), et de situer la linguistique dans les cadres d'une sémiotique (ou sémiologie) générale. Les définitions exactes que nous proposons pour *système sémiotique* et pour *langue* (*langage*) ont été données ailleurs[3].

Cette doctrine ne constitue d'ailleurs qu'une hypothèse de travail qui, nous l'espérons, aidera à trouver l'axiomatique qui s'adapte le mieux.

Cette doctrine, et la double distinction fondamentale, mentionnée plus haut, sur laquelle elle repose, sont aujourd'hui sujettes à discussion. En acceptant l'invitation à collaborer au présent volume par une contribution portant sur la théorie et la méthode de la glossématique, nous avons donc estimé ne pouvoir mieux faire que de présenter sur la double distinction qui vient d'être signalée quelques réflexions que nous croyons aptes à éclaircir plus amplement la question, à fixer utilement les idées et à apporter certaines précisions que nos derniers

[3] L'auteur, *Prolegomena to a Theory of Language*, 1953, p. 85 sv., définitions n°s 53 et 88. Version française de ces définitions (accompagnée d'une appréciation critique) par A. Martinet dans *Bulletin de la Société de linguistique de Paris* XLII, 1, p. 33.

résultats permettent d'y ajouter. Pour éviter les redites et les banalités nous supposerons connus les contours essentiels de la théorie tels qu'ils ont été exposés ailleurs[4]. On ne s'attendra donc pas à retrouver dans ce qui va suivre une introduction à la glossématique; il s'agira au contraire d'une contribution qui vise à faire comprendre un problème plus restreint, mais essentiel, peut-être le plus essentiel dans cet ordre d'idées.

Bien que ce soit manifestement mettre du vin nouveau dans de vieilles outres, nous nous sommes permis, non sans scrupules, de reproduire pour notre exposé le titre d'un travail publié en 1869 par Max Müller: *La stratification du langage*. Il est vrai que la «stratification» à laquelle nous pensons ne s'identifie nullement à celle que le vénérable Oxonien a voulu mettre en lumière. Nous croyons cependant que, malgré cette fortuite coïncidence, le titre se trouvera justifié. *Stratification* se présente comme le terme le plus naturel pour rendre notre idée; avouons seulement que, sous peine de courir le risque d'être accusé d'inexactitude en matière de terminologie, nous aurions dû mettre: *La stratification du système sémiotique*[5].

En effet, une des thèses que nous allons soutenir (et que l'on trouvera dans les dernières pages du présent exposé) implique, à certains égards, une relation analogue entre la substance du contenu, la forme du contenu, la forme de l'expression et la substance de l'expression, si bien que, si on passe dans l'ordre indiqué (en avant ou en arrière) de l'un de ces quatre compartiments à l'autre, on peut faire pour chaque pas les mêmes observations. Il paraît possible d'énoncer des lois dirigeant les rapports entre ces quatre grandeurs, en les prenant deux à deux, lois qui s'avéreront indifféremment valables pour n'importe laquelle de ces paires. On a par conséquent intérêt à disposer d'un nom commun pour les désigner. C'est un fait digne d'attention d'ailleurs que, dans la terminologie consacrée depuis le *Cours* de Saussure, on dispose du mot *plans* pour désigner le contenu (le signifié) et l'expression (le signifiant), mais non d'un terme commun servant à désigner les quatre grandeurs que nous envisageons. Nous proposons de les appeler *strata*.

Nous jugerons utile pour notre but de mettre ces quatre strata sur le même pied, et de les regarder d'un angle qui les rende coordonnés, ce qui permettra de les manier librement et sans idées préconçues, de les classer de différentes manières, et de comparer à son gré, et de tous les points de vue, les classes de strata ainsi obtenues.

De la sorte on arrivera à constater entre les classes de strata des différences et des analogies. Nous traiterons d'abord, assez longuement, des différences, pour finir par insister sur les analogies.

[4] L'auteur, *Prolegomena* (voir note 3). Une traduction française de cet ouvrage (paru d'abord en danois en 1943), sous le titre de *Prolégomènes à une théorie du langage*, est actuellement sous presse dans les *Travaux du Cercle ling. de Copenhague*. — L'index de termes et de définitions qu'on trouve à la fin de ce travail nous dispensera la plupart du temps de renvois plus détaillés.

[5] A condition d'y comprendre la progression (en anglais: *process*) (le texte), qui détermine le système (la langue) (v. *Prolegomena*, p. 24).

Avant d'entrer dans ces démonstrations, nous avons cependant une tâche pratique à accomplir, qui se trouvera utile et même indispensable: celle d'introduire des désignations univoques pour chacun des strata. Le plus simple et le plus facile est de se servir d'un jeu de symboles analytiques.

D'une façon générale, dans le système de symboles qui sera utilisé en glossématique, les grandeurs du contenu (dites aussi grandeurs plérématiques) sont symbolisées par des caractères grecs, les grandeurs de l'expression (dites aussi grandeurs cénématiques) par des caractères latins (italiques ou romains); pour symboliser une grandeur appartenant à n'importe lequel des deux plans, on se sert d'un caractère latin précédé d'un astérisque au bas de la ligne. Par exemple, $*g$ est le symbole d'un glossème (invariante irréductible), tandis que γ symbolise spécialement un glossème du contenu (ou plérématème), et g un glossème de l'expression (ou cénématème). De même, $*G$ signifie taxème, Γ taxème du contenu, G taxème de l'expression.

Pour désigner la manifestation (la relation spécifique entre forme et substance) on se sert des signes V et Λ. Ces symboles ont été choisis pour évoquer le mot *valeur:* on peut dire en effet (par analogie avec la valeur d'échange des sciences économiques[6]) qu'une grandeur donnée qui relève de la forme constitue la valeur qui dans le système sémiotique considéré est attribuée à la grandeur de substance qui lui correspond et par laquelle elle est manifestée. Par exemple, on peut dire que la grandeur phonique [p] a la valeur p (où p représente un taxème d'expression d'un état de langue donné), ou, pour prendre un exemple où la différence entre forme et substance se révèle plus nettement dans la notation, que, en français, la grandeur phonique [˜] (nasalité) a, dans certains cas démonstrables, la valeur d'une variante (définie) du taxème n (comme dans le mot qui s'écrit «bon»)[7]. On écrit dans ces cas

$$p \text{ V } [\text{p}]$$
$$\text{var. } (n) \text{ V } [\tilde{}]$$

ou, s'il y a lieu de renverser les termes,

$$[\text{p}] \text{ Λ } p$$
$$[\tilde{}] \text{ Λ var. } (n)$$

Donc, V signifie 'manifesté (par)' ou 'forme (par rapport à)', et Λ signifie 'manifestant' ou 'substance (par rapport à)'.

Si, en outre, par une disposition arbitraire, le signe ° est choisi pour désigner un stratum ou une classe de strata, on obtiendra les symboles suivants qui seront utilisés dans la suite:

$*g°$ = la forme sémiotique (forme du contenu ou forme de l'expression, forme sans égard au plan ou dans les deux plans indifféremment, et comprise dans son ensemble)

[6] V. l'auteur, *Langue et parole* (*Cahiers Ferdinand de Saussure* 2, 1943) p. 39, et *Structural Analysis of Language* (*Studia Linguistica*, année 1, 1947, n° 2), p. 70.

[7] K. Togeby, *Structure immanente de la langue française* (*Travaux du Cercle ling. de Copenhague* VI, 1951), p. 58.

γ° = la forme du contenu (dans son ensemble)
g° = la forme de l'expression (dans son ensemble)
$\Lambda{*}g^{\circ}$ = la substance sémiotique (substance du contenu ou substance de l'expression, substance sans égard au plan ou dans les deux plans indifféremment, et comprise dans son ensemble)
$\Lambda\gamma^{\circ}$ = la substance du contenu (dans son ensemble)
Λg° = la substance de l'expression (dans son ensemble)

Pour des buts particuliers, il est naturel d'introduire pour les substances de l'expression des symboles comme les suivants (qui peuvent être multipliés selon les besoins):

Λg_{f} = grandeur phonique manifestant un glossème de l'expression (ou cénématème)
ΛG_{f} = phonématème (grandeur phonique manifestant un taxème de l'expression)
Λg°_{f} = substance phonique (phonématique)
Λg_{g} = grandeur graphique manifestant un glossème de l'expression (cénématème)
ΛG_{g} = graphématème (grandeur graphique manifestant un taxème de l'expression)
Λg°_{g} = substance graphique (graphématique)

Si l'on désigne la facultativité par des parenthèses, on peut indiquer par (V) l'absence possible de manifestation qui est due au fait que la forme est sélectionnée par la substance. On obtient dès lors

γ°(V) = plan du contenu
g°(V) = plan de l'expression
${*}g^{\circ}$(V) = plan

S'il y a lieu, on peut facilement élargir ces symboles de diverses façons.

Ainsi, paradigmatique (ou système sémiotique) et syntagmatique (ou progression sémiotique) se distinguent naturellement par les signes de corrélation (:) et de relation (R) respectivement. En usant d'un système de symboles précis, cette distinction est évidemment de rigueur.

De plus, on peut désigner par une L une «sémiotique linguistique» (c'est-à-dire une «langue» dans le sens traditionnel des linguistes, et en y comprenant le texte, ou syntagmatique linguistique).

C'est ainsi qu'on peut écrire, s'il y a lieu:

$\gamma^{\circ}g^{\circ}$(V): = une paradigmatique
$\gamma^{\circ}g^{\circ}$(V)R = une syntagmatique
$\gamma^{\circ}g^{\circ}$(V) = une sémiotique (c.-à-d. l'ensemble d'une paradigmatique et d'une syntagmatique)
L$\gamma^{\circ}g^{\circ}$(V): = langue (dans le sens restreint d'un *système* linguistique)
L$\gamma^{\circ}g^{\circ}$(V)R = texte
L$\gamma^{\circ}g^{\circ}$(V) = une sémiotique linguistique

Il parait malaisé, et même indésirable, de traduire ces symboles et leurs combinaisons dans une terminologie, ou de les supplanter par des termes plus ou

moins artificiels. Au contraire, les symboles et leurs combinaisons sont utiles même dans les cas où l'on dispose déjà d'un terme consacré, vu que les termes consacrés ne manquent pas d'ambiguïté. Par exemple, l'introduction des symboles fait voir immédiatement que, malgré les nuances subtiles qui sont rendues possibles par la délicate distinction du français entre *langue* et *langage*, ces termes n'admettent nullement l'univocité absolue qui sera indispensable pour le but que nous nous proposons ici. L'analyse que nous allons entreprendre fera voir aussi, entre autres choses, l'avantage qu'il y a à symboliser d'une façon brève et simple la forme et la substance *sémiotiques*, et à éviter de la sorte les ambiguïtés des termes généraux de *forme* et de *substance*.

Pour la commodité de notre exposé, et pour éviter les complications inutiles, il sera supposé ci-après en principe que l'objet d'investigation soit une seule sémiotique (et non pas, par exemple, une famille ou une succession génétique de langues, ni une classe typologique renfermant deux ou plusieurs langues, ni un agglomérat de langues différentes représentées, comme c'est souvent le cas, dans un seul et même texte choisi comme objet d'analyse). De plus, il sera supposé également, sauf indication contraire, que la sémiotique envisagée ne soit ni une sémiotique connotative ni une métasémiotique. De fait et pratiquement, le lecteur pourra se représenter un état de langue ordinaire, ou, au besoin, une sémiotique non-linguistique qui, du point de vue choisi et en principe, ressemble le plus à un état de langue ordinaire.

D'ailleurs les observations que nous allons faire s'appliquent à n'importe quelle sémiotique ou classe de sémiotiques, y comprises les sémiotiques connotatives, les métasémiotiques, les classes typologiques, familles génétiques etc. etc. Pour sauvegarder une méthode empirique, l'analyste doit prévoir toutes les possibilités et maintenir une attitude agnostique vis-à-vis de la classe qui, à chaque stade, fait l'objet de l'analyse. Si l'objet de l'anlayse est une hiérarchie, c'est-à-dire une classe de classes, il peut se révéler comme une hiérarchie de hiérarchies, sans conformité entre elles; donc, dès le moment où, à un certain stade de l'analyse de cet objet complexe, un manque de conformité se dénonce, l'analyste doit reconnaître l'existence de deux hiérarchies différentes, et procéder par conséquent, en vue d'épuiser l'analyse complète, à deux analyses séparées. C'est ainsi que, si l'objet de l'analyse est une sémiotique (c'est-à-dire si au cours de l'analyse il se trouve satisfaire à la définition d'une sémiotique), il faut distinguer les deux plans et les analyser séparément, à partir du moment même où, pendant l'analyse de l'ensemble, ils révèlent entre eux une différence de structure; de même, dans le cas d'une sémiotique manifestée, il faut distinguer et analyser séparément forme et substance dès le moment où pendant l'analyse de l'ensemble elles cessent d'être mutuellement conformes. C'est ainsi qu'on distingue aussi les deux plans d'une métasémiotique (dont le plan de contenu se révèle comme étant une sémiotique) et ceux d'une sémiotique connotative (dont le plan d'expression se révèle comme étant une sémiotique), aussi bien que les structures divergentes qui, au fur et à mesure que l'analyse d'un texte progresse, se révèlent comme étant des langues différentes, dont les conformités s'expliquent ou bien par une relation spécifique génératrice de l'emsemble (d'ordre typologique ou génétique)

ou bien par une rencontre plus fortuite dans un seul et même texte; et ainsi de suite. Le principe reste toujours le même: l'exigence d'une analyse exhaustive veut qu'on distingue ce qui manque de conformité, et que la séparation soit accomplie dès le moment où le manque de conformité se dénonce; le principe de la simplicité veut, d'autre part, qu'on ne distingue pas avant ce moment, sous peine d'opérer des distinctions inutiles.

Le principe s'applique donc indifféremment à n'importe quelle classe de hiérarchies. Il s'ensuit qu'il s'applique, plus particulièrement, aux classes constituées par ces hiérarchies que nous avons appelées les strata, et qui s'observent le plus facilement dans une sémiotique ordinaire, ou dans un état de langue ordinaire. On pourra donc bien, pour simplifier, commencer par se borner à ces cas. Il est vrai d'autre part qu'une telle limitation est un artifice qui la plupart du temps correspond mal aux faits empiriques tels qu'ils se présentent le plus souvent au linguiste, puisque dans un état de langue ordinaire les connotations s'imposent constamment. Mais l'artifice est innocent, parce que l'uniformité du principe rendra les généralisations faciles à opérer.

Dans notre examen des différences entre les classes de strata, il paraît naturel de commencer par les classifications des strata qui recouvrent les deux distinctions saussuriennes: celle entre γ^{o}(V) et g^{o}(V) (qui est la distinction des deux plans) et celle entre $*g^{o}$ et $\Lambda*g^{o}$ (ou entre forme et substance).

On peut faire observer d'abord qu'une première différence entre ces deux distinctions est dans le stade d'analyse où elles s'imposent: la distinction entre contenu et expression est supérieure à celle entre forme et substance, si bien que, dans la procédure de l'analyse, la bifurcation qui conduit à séparer la hiérarchie constituée par le plan du contenu et celle constituée par le plan de l'expression se trouve à un stade antérieur à celle qui sépare forme et substance. C'est pourquoi il faut parler, comme nous l'avons fait déjà en effet et comme nous aurons à le faire dans la suite, de la 'forme du contenu', de la 'substance du contenu', de la 'forme de l'expression' et de la 'substance de l'expression', alors qu'il serait insensé, parce qu'inadéquat, de parler d'un 'contenu de substance', d'un 'contenu de forme', d'une 'expression de substance', ou d'une 'expression de forme'. La distinction entre contenu et expression est le premier carrefour, celle de forme et substance le second, et la distinction de forme et de substance est donc subordonnée à celle entre les plans.

De la sorte, la distinction des plans se greffe sur celle des substances, et la deuxième distinction saussurienne se traduira non par la simple distinction entre $*g^{o}$ et $\Lambda*g^{o}$, mais par la distinction entre γ^{o} et $\Lambda\,\gamma^{o}$ d'une part, et par celle entre g^{o} et Λg^{o} de l'autre. Il n'y a pas de fonction (ou dépendance immédiate[3]) entre $\Lambda\gamma^{o}$ et g^{o}, ni entre $\Lambda\, g^{o}$ et γ^{o}, ni d'ailleurs entre $\Lambda\gamma^{o}$ et Λg^{o}. En d'autres termes, la multiplication des deux distinctions saussuriennes conduit déjà à établir exactement trois classes de strata: 1° γ^{o}(V) et g^{o}(V), 2° γ^{o} et $\Lambda\gamma^{o}$, 3° g^{o} et Λg^{o}.

Les classes de cette sorte se laissent concevoir du point de vue syntagmatique ou du point de vue paradigmatique, donc comme des chaînes ou comme des

[3] V. l'auteur, *La structure morphologique* (*V^e Congrès international des Linguistes, Rapports*, 1939), p. 67.

paradigmes, respectivement. Il est vrai que, en l'espèce, c'est la conception syntagmatique qui s'impose le plus, parce que les strata se présentent à l'analyse immédiate comme coëxistant, et que par conséquent la fonction génératrice d'une classe de strata est pour l'analyse immédiate une relation (ou conjonction logique) entre les strata compris dans la classe. Aussi la théorie a-t-elle jusqu'ici insisté d'une façon exclusive, bien que toute naturelle, sur les relations entre strata; on les retrouvera d'ailleurs dans un instant. Mais pour être complet il faut y suppléer en concevant les strata comme alternant, donc comme les membres d'un paradigme dont la fonction génératrice est une corrélation. Puisque ce point de vue ne constitue qu'un corollaire qui ne présente que rarement un véritable intérêt pratique, nous ne ferons que le mentionner brièvement. Indiquons simplement que, de ce point de vue, les plans paraissent être mutuellement autonomes, tandis que, à l'intérieur de chaque plan, forme et substance sont mutuellement complémentaires; donc, les trois classes de strata obtenues par la multiplication des deux distinctions saussuriennes se définissent, si elles sont conçues comme des paradigmes, selon les formules que voici[9]:

$$\gamma^{o}(\mathrm{V}) \dagger g^{o}(\mathrm{V})$$

$$\gamma^{o} \downarrow \Lambda\gamma^{o}$$

$$g^{o} \downarrow \Lambda g^{o}$$

Mais, nous l'avons dit, les relations entre strata offrent un intérêt beaucoup plus considérable que les corrélations. Or, de ce point de vue encore, il y a différence entre les deux distinctions saussuriennes. La relation qui réunit les deux plans (la *relation sémiotique*, ou, plus spécialement, dans le cas d'une sémiotique ordinaire, la *dénotation*) est, on le sait, une solidarité, tandis que la relation entre forme et substance (qui s'appelle *manifestation*) est une sélection, la substance sélectionnant (manifestant) la forme.

Mais ces définitions admettent des précisions. Prises au pied de la lettre et telles quelles, elles se traduiraient par les formules

$$\gamma^{o}(\mathrm{V}) \backsim g^{o}(\mathrm{V}) \qquad (1)$$

$$\Lambda{*}g^{o} \rightarrow {*}g^{o} \qquad (2)$$

Or, on sait déjà par ce qui précède que la formule (2) doit être scindée en deux:

$$\Lambda\gamma^{o} \rightarrow \gamma^{o}$$

$$\Lambda g^{o} \rightarrow g^{o}$$

Pour ce qui est de la formule (1), il faut se rendre compte du fait que, tout en restant la distinction subordonnée comme nous l'avons vu, la distinction entre forme et substance se greffe aussi sur celle entre contenu et expression, ainsi qu'il est indiqué par le symbole (V) qui entre dans la formule générale ${*}g^{o}(\mathrm{V})$. Puisque la forme est, à l'intérieur de chaque plan, sélectionnée par la substance,

[9] Pour les termes et symboles utilisés pour désigner les diverses fonctions, le lecteur est prié de se reporter aux *Prolegomena*, surtout p. 25, note.

il est possible en partie (mais non, croyons-nous, de tous les points de vue), de concentrer les relations entre les deux plans (et des grandeurs relevant de deux plans différentes) de façon à considérer ces relations comme contractées par la forme du contenu (ou par des grandeurs qui en relèvent) et la forme de l'expression (ou par ses grandeurs) simplement, en faisant abstraction dans les deux plans de la substance. Il est de première importance pratique de savoir quelles sont les relations qui permettent une telle concentration.

La réponse à cette question n'est pas loin. Puisque la détermination (fonction unilatérale entre la substance comme variable et la forme comme constante) n'est valable que du seul point de vue syntagmatique (comme une sélection), tandis que du point de vue paradigmatique il y a réciprocité (plus particulièrement: complémentarité) entre forme et substance, la substance ne peut jouer le rôle d'une variable que dans les cas nets où pour l'analyse immédiate le syntagmatique est seul en cause. Dans ces cas donc, il paraît certain que le champ de relation entre les plans peut être concentré de la façon indiquée, c'est-à-dire à ne comprendre que la forme seule, tandis qu'on peut prévoir d'autres cas, rendus plus compliqués par le fait d'un concours entre syntagmatique et paradigmatique, et où par conséquent la substance doit être comprise dans les deux plans (ou dans les grandeurs qui en relèvent) pour définir les fonctions qu'ils contractent entre eux.

Il paraît certain que l'interdépendance constituée par la fonction sémiotique est d'ordre nettement syntagmatique (donc, comme nous l'avons dit, une solidarité), et que, en conséquence de ce fait, la relation sémiotique doit être considérée comme contractée par la forme du contenu et la forme de l'expression seules, sans le concours des substances, si bien que la formule (1) sera remplacée désormais par la formule plus simple et plus exacte

$$\gamma^{\circ} \backsim g^{\circ}$$

Faisons remarquer tout de suite que le fait que la corrélation entre les plans paraît pouvoir se définir comme une autonomie ne sert pas à compliquer la situation: du point de vue paradigmatique aussi bien que du point de vue syntagmatique, les plans ne contractent qu'une réciprocité, et il n'y a pas pour les plans le conflit entre le paradigmatique et le syntagmatique qui s'observe pour forme et substance.

Il paraît, d'autre part, que la *commutation*, qui est, pour l'analyse immédiate et d'ailleurs de tous les points de vue, une corrélation (qui contracte une relation avec une corrélation du plan opposé), et, d'une façon plus générale, les corrélations entre variantes qui, à n'importe quel stade de l'analyse de chaque plan, permettent l'identification[10] des éléments, constituent le domaine propre dans lequel le concours de la substance (dans le cas où elle existe) s'impose. La question ne se pose d'ailleurs qu'en des circonstances particulières, à savoir pour la vérification d'un objet empirique, tel qu'une langue qui fait l'objet d'une enquête; pour les autres cas (structures résultant d'un pur calcul, ou d'une reconstitution

[10] Dans le sens de «l'identification d'un criminel». V. Lalande, *op. cit.*, III, p. 69.

génétique) la substance, même si elle est introduite, est construite à dessein de façon à satisfaire aux exigences de commutations et d'identités numériques prévues par la forme. Donc, dès qu'il s'agit de la commutation, et des identifications d'éléments qui en dérivent, le plan ne se réduit pas à la forme pure, mais se définit comme $*g^{o}(V)$: $\gamma^{o}(V)$ et $g^{o}(V)$ respectivement.

Nous croyons que les précisions que nous venons de donner, à la base du système de formules plus exactes que nous avons introduit, permettront de mieux comprendre la position du problème, souvent débattu, des rapports entre forme et substance au sein de la structure sémiotique. Il y aura encore d'autres précisions à apporter dans la suite, et on parlera encore de certaines différences entre les strata avant d'aborder la question des analogies. Arrêtons-nous cependant un instant pour appeler l'attention sur certaines conséquences plus générales de la distinction entre forme et substance telle qu'elle a été dès maintenant précisée.

Les formules que nous avons choisies pour rendre les termes de 'forme' et de 'substance', et pour préciser une certaine ambiguïté trouvée dans le terme 'plan' (comme impliquant ou non la manifestation possible), ne sont valables que pour la forme et la substance *sémiotiques*. Mais les termes de 'forme' et de 'substance', tels qu'ils ont été introduits par F. de Saussure, admettent sans doute une application plus générale. Il est probable que toute analyse scientifique, de n'importe quel objet (considéré dès lors comme une classe dans notre sens de ce mot), implique par nécessité la distinction entre deux strata, ou hiérarchies, que l'on peut identifier à la forme et la substance dans l'acception saussurienne (mais générale) de ces termes. La «forme», dans ce sens général, se définit comme l'ensemble total, mais exclusif, des marques qui, selon l'axiomatique choisie, sont constitutives des définitions[11]. Tout ce qui n'est pas compris dans une telle «forme», mais qui de toute évidence appartiendrait à une description exhaustive de l'objet étudié, est rélégué à une autre hiérarchie qui par rapport à la «forme» joue le rôle de «substance». Forme et substance sémiotiques ne constituent en effet qu'un cas particulier de cette distinction générale. Nous avons dans un autre travail[12] appelé l'attention sur une telle généralisation possible, mais en soulignant en même temps le fait, sur lequel nous insistons ici encore, que, dès le moment où l'on change de point de vue et procède à l'analyse scientifique de la «substance», cette «substance» devient forcément à son tour une «forme», d'un degré différent il est vrai, mais une «forme» néanmoins, dont le complément est encore une «substance», comprenant encore une fois les résidus qui n'ont pas été acceptés comme les marques constitutives des définitions. Ceci revient à dire que dans ce sens général «forme» et «substance» sont des termes *relatifs*, non des termes absolus.

De ce point de vue, il serait évidemment injuste de prétendre, comme nous l'avons fait plus haut, que la distinction entre forme et substance soit su-

[11] On serait tenté de dire marques *pertinentes*, si ce n'était que, en linguistique moderne, ce mot a été réservé à signifier 'ce qui dans la substance est (supposé) pertinent pour une différence dans la forme'.

[12] *Structural Analysis of Language*, p. 75. Cf. *Prolegomena*, p. 79.

bordonnée à celle entre contenu et expression. Au contraire, des deux distinctions saussuriennes, telles qu'elles ont été transmises dans le *Cours*, la distinction des plans s'applique à la seule sphère sémiotique—elle est spécifique à la sémiotique au point même de la définer—alors que la distinction entre «forme» et «substance» paraît être d'une application beaucoup plus générale: il semble s'agir tout simplement de l'*abstraction*, qui est le rançon de toute analyse scientifique.

Il ne faut pas se dissimuler le fait que, de ce point de vue, la terminologie saussurienne peut prêter à la confusion. On ne saurait y remédier que par les artifices que nous avons proposés, et qui consistent à réserver, en matière sémiotique, par une disposition arbitraire, les termes de 'forme' et de 'substance' à ne désigner que forme et substance *sémiotiques*, c.-à-d. $*g^{o}$ et $\Lambda*g^{o}$ respectivement, ou, plus exactement encore, γ^{o} et $\Lambda\gamma^{o}$, g^{o} et Λg^{o} selon les deux plans dans lesquels elles sont dès lors forcément ancrées.

Ce qui nous reste à dire encore sur les différences entre (classes de) strata aura trait exclusivement à la différence entre forme et substance, et servira donc en même temps à élucider certaines analogies entre γ^{o} et g^{o} d'un côté, et entre $\Lambda\gamma^{o}$ et Λg^{o} de l'autre.

Il est vrai que ce que nous en dirons est d'un caractère largement hypothétique, et ne constitue qu'un premier sondage dans un terrain à peine défriché. Ceci ne laissera certainement pas de surprendre le lecteur non averti, vu que la question dont il s'agit est très fondamentale, voire même élémentaire, à savoir la question des grands principes de la structure interne de la substance. C'est une situation en effet qui fait quelque peu rêver, car si on prétend souvent, spécieusement, que la substance, et surtout celle de l'expression qui a constitué l'objet favori des recherches linguistiques pendant un siècle au moins, est mieux connue que la forme sémiotique, l'abîme d'ignorance devant lequel nous nous trouvons à l'état actuel des recherches fait comprendre, d'une façon particulièrement impressionnante, que la linguistique n'en est encore qu'à ses premiers débuts. Nous estimons cependant que le seul moyen pour y remédier et pour contribuer aux progrès consiste à émettre des hypothéses: *citius emergit ueritas ex errore quam ex confusione*.

Les hypothèses s'imposent d'autant plus qu'elles sont suscitées inévitablement par certains faits évidents qui sautent aux yeux et qui demandent une explication.

Un de ces faits, et peut-être le plus saillant, est la multiplicité des substances. C'est un fait bien connu, par exemple, qu'une seule et même forme de l'expression peut être manifestée par des substances diverses: phonique, graphique, signaux par pavillons, etc. On hésite souvent devant ce fait; on l'explique de façons diverses. On peut le discuter, mais on ne peut pas l'écarter. Le fait reste.

A propos de ce fait il semble utile de faire trois observations.

D'abord, s'il est dit qu'une même forme sémiotique peut revêtir des substances diverses, il importe de comprendre que, dans la terminologie glossématique que nous utilisons ici, *substance* veut dire substance *sémiotique*: $\Lambda*g^{o}$. La substance est déjà sous la domination de la forme sémiotique ou $*g^{o}$. C'est une substance *sémiotiquement formée*. Or, pour paradoxale que cela puisse paraître à première vue, c'est précisément cette notion particulière de substance qui nous sera utile

pour faire ressortir le rapport entre forme et substance. Un exemple le fera voir. Dans le cas normal d'une langue telle que le français ou l'anglais, l'analyse phonématique et l'analyse graphématique du plan de l'expression ne feraient que fournir deux formes sémiotiques différentes, et non une seule et même forme sémiotique manifestée par des substances différentes. Ce cas n'offre donc pas l'exemple désiré. Pour l'obtenir, il faut que la forme sémiotique reste identique à elle-même, mais qu'elle soit manifestée différemment. Des exemples valables sont fournis par une prononciation (donc substance phonématique) et une notation phonologique correspondante (ou une orthographe susceptible d'en faire fonction) (donc substance graphématique), ou bien par une orthographe (donc substance graphématique) et un système de signaux par pavillons où chaque signal, ou chaque pavillon, correspond à un seul graphème (lettre de l'alphabet par exemple; donc, substance «mappématique», s'il nous est permis d'introduire le terme) ou une dactylolalie comme celle des sourds-muets (substance que nous appelons «gérématique»). Dans les exemples de ce genre il faut pouvoir dire que, malgré le fait (inévitable) que la substance reflète la forme sémiotique, plusieurs substances concourent pour manifester la forme. On voit du même coup d'ailleurs qu'il sera impossible de renverser les termes, et de prétendre qu'une même substance puisse revêtir des formes sémiotiques différentes.

Pour désigner la manifestante sans impliquer qu'elle soit sémiotiquement formée, c'est-à-dire sans distinguer manifestante sémiotiquement formée et manifestante sémiotiquement non-formée, ce qui est une notion entièrement différente, nous proposons le terme *matière*[13]. Entre autres choses, ce terme est fait exprès pour permettre de dire qu'une même matière (p. ex. matière phonique, graphique etc.) peut servir à manifester des formes sémiotiques différentes, ce qui est autre chose, et par conséquent sans contradiction avec ce qui vient d'être dit. Ajoutons d'ailleurs que, sous peine d'échapper à la connaissance, cette matière doit être scientifiquement formée, du moins à un degré qui permette de la distinguer d'autres matières; toute science est une sémiotique, il est vrai, mais d'un ordre différent de celui qui nous occupe; que ce soit dit simplement pour être complet, et en même temps pour prévenir à la confusion.

Ensuite, pour bien comprendre la multiplicité possible des substances par rapport à la forme sémiotique, il faut se rendre compte du fait que la règle donnée vaut pour la forme de chaque plan pris à part: pour γ° et pour g° respectivement. Or il convient de rappeler à ce propos le caractère arbitraire de la fonction sémiotique qui réunit γ° et g°; ce fait, qui révèle une analogie entre la fonction sémiotique et la manifestation, et sur lequel nous aurons à revenir, signifie qu'une même forme de contenu peut être exprimée par plusieurs formes d'expression, et inversement, fait responsable justement de la situation que l'on vient de constater pour les systèmes d'expression, à manifestation graphique et phonique respectivement, de l'anglais et du français. Mais même la forme d'expression dont la manifestation coutumière est d'ordre phonique peut, on le sait, pour l'anglais ou le français être rendue par une notation phonologique, donc en utilisant une substance différente; de même, l'alphabet anglais ou français peut être rendu par

[13] Ou *sens*; en anglais: *purport*. *Prolegomena*, p. 31.

des signaux par pavillons, et ainsi de suite. Donc, ce qui vaut pour une langue dont l'usage ne permet qu'une seule forme de l'expression vaut au même titre pour chacune des formes d'expression (c-.à-d. pour chaque $*g^{\circ}$) d'une langue qui, comme le français ou l'anglais, admet plusieurs formes de cet ordre: une seule et même forme peut revêtir des substances diverses, mais non inversement.—Ajoutons d'ailleurs, pour être complet, que, théoriquement du moins, une sémiotique peut comporter plus de deux plans. Mais ce fait, bien que prévisible d'un point de vue purement calculatoire, ne change rien au principe, qui reste le même, à savoir que chaque forme ou $*g^{\circ}$ prise à part, c'est-à-dire $*g^{\circ}_1$ ou γ°, $*g^{\circ}_2$ ou g°, $*g^{\circ}_3$, $\cdots$, $*g^{\circ}_n$, peut revêtir plus d'une substance, mais non inversement.

Enfin, on ne saurait signaler le fait constitué par la multiplicité des substances sans insister, tout d'une haleine, sur un autre fait qui vient le compliquer apparemment: nous voulons dire le fait qu'une même substance comporte à son tour plusieurs aspects, ou, comme nous préférons dire, plusieurs *niveaux*. C'est un fait très important, mais fort mal étudié. On sait que la substance phonique, prise dans son ensemble et dans le sens le plus large du terme, demande tout au moins une description physiologique (dite aussi articulatoire, myocinétique, etc.) et une description purement physique (ou acoustique, dans le sens propre de ce terme), et qu'il faut y ajouter sans doute une description auditive, selon l'apperception des sons du langage par les sujets parlants. En principe, les autres substances d'expression concevables ne se comportent pas autrement: il y aura, pour elles également, tout au moins une description physique et une description par apperception. La substance du contenu ne paraît pas être exempt d'une différenciation analogue: on ne sépare pas utilement les grandeurs sémantiques susceptibles d'une description physique (telles que 'cheval', 'chien', 'montagne', 'sapin') et celles qui se prêtent surtout, ou peut-être même uniquement, à une description utilisant les termes d'apperception ou d'évaluation (telles que 'grand', 'petit', 'bon', 'mauvais'). Au contraire, à regarder la substance du contenu dans son ensemble et du dehors, les deux points de vue se confondent et se suppléent constamment, bien qu'à des degrés divers, et il y a un glissement constant entre eux. Ce n'est pas dire qu'il ne faille pas les distinguer; seulement séparer et distinguer n'est pas le même chose. A l'analyse (qui n'est pas nécessairement une partition ou une division) le tout se présente justement comme comportant divers aspects ou niveaux qui se correspondent et se complètent et que l'analyse doit dégager. Elle ne saurait le faire sans se rendre compte d'un certain ordre hiérarchique de ces niveaux, et ce fait est peut-être, à première vue, plus évident et plus facile à saisir pour le contenu que pour l'expression. De toute évidence c'est la description par évaluation qui pour la substance du contenu s'impose immédiatement. Ce n'est pas par la description physique des choses signifiées que l'on arriverait à caractériser utilement l'usage sémantique adopté dans une communauté linguistique et appartenant à la langue qu'on veut décrire; c'est tout au contaire par les évaluations adoptées par cette communauté, les appréciations collectives, l'opinion sociale. La description de la substance doit donc consister avant tout en un rapprochement de la langue aux autres institutions sociales, et constituer le point de contact entre la linguistique et les

autres branches de l'anthropologie sociale. C'est ainsi qu'une seule et même «chose» physique peut recevoir des descriptions sémantiques bien différentes selon la civilisation envisagée. Cela ne vaut pas seulement pour les termes d'appréciation immédiate, tels que 'bon' et 'mauvais', ni seulement pour les choses créées directement par la civilisation, telle que 'maison', 'chaise', 'roi', mais aussi pour les choses de la nature. Non seulement 'cheval', 'chien', 'montagne', 'sapin' etc. seront définis différemment dans une société qui les connaît (et les *re*connaît) comme indigènes et dans telle autre pour laquelle elles restent des phénomènes étrangers—ce qui d'ailleurs n'empêche pas, on le sait bien, que la langue dispose d'un nom pour les désigner, comme p. ex. le mot russe pour l'éléphant: *slon*. Mais l'éléphant est quelque chose de bien différent pour un Hindou ou un Africain qui l'utilise et le cultive, qui le redoute ou qui l'aime, et d'autre part pour telle société européenne ou américaine pour laquelle l'éléphant n'existe que comme un objet de curiosité exposé dans un jardin d'acclimatation et dans les cirques ou les ménageries, et décrit dans les manuels de zoologie. Le 'chien' recevra une définition sémantique tout à fait différente chez les Eskimos, où il est surtout animal de trait, chez les Parses, dont il est l'animal sacré, dans telle société hindoue où il est réprouvé comme paria, et dans nos sociétés occidentales dans lesquelles il est surtout l'animal domestique dressé pour la chasse ou pour la vigilance.

Il est vrai que ces considérations ne concernent pas que la substance, et qu'elles ont de graves répercussions sur l'analyse formelle des unités en question. Il n'en reste pas moins que la substance immédiate du contenu semble consister en des éléments d'appréciation; on peut même dire que, grace à cette analyse formelle, le niveau immédiat de la substance se réduira à plus forte raison à n'être que d'une nature nettement appréciative.

En somme, un examen provisoire de la substance du contenu invite à conclure que à l'intérieur de cette substance le niveau primaire, immédiat, parce que seul directement pertinent du point de vue linguistique et anthropologique, est un niveau d'appréciation sociale. Ce résultat a été gagné surtout par un examen du langage, mais pourra facilement être généralisé de façon à valoir en principe pour n'importe quelle sémiotique. Pour le dire en passant, la méthode qui consiste à décrire le niveau d'appréciation sociale présente aussi l'avantage, indispensable au linguiste, de pouvoir rendre suffisamment compte des «métaphores», qui dans quelques cas jouent un rôle au moins aussi considérable que le «sens propre», et qui le plus souvent proviennent justement d'une telle appréciation collective qui arbitrairement met en évidence certaines qualités attribuées de préférence (souvent alternativement, par syncrétisme) à l'objet envisagé (par exemple si, dans telle ou telle langue, 'renard' peut signifier un 'homme rusé' ou un 'homme roux')[14]. Les terminologies scientifiques sont pour une large part créées exprès pour

[14] A ce propos une remarque faite par Vilh. Grønbech nous vient á l'esprit: «Je sens immédiatement ce que c'est que de se risquer dans l'antre du lion en entrant chez le chef du bureau, je sais entendre le rugissement de lion provenant du tyran et du démagogue, et admirer le courage de lion et l'orgueil de lion manifestés par le héro; j'ai connu bien des façons burlesques et des tracasseries de ce renard qu'est Monsieur Untel; et c'est par lá

éviter de telles implications métaphoriques ou appréciations collectives et traditionnelles; elles n'y arrivent souvent que partiellement, et, à moins de se cantonner dans un fourré de formules absolument arbitraires, elles partagent en principe le sort d'une langue naturelle.

Il paraît donc que le premier devoir du linguiste, ou, plus généralement, du sémiotiste, qui voudrait entreprendre une description de la substance du contenu consisterait à décrire ce que nous avons appelé le niveau de l'appréciation collective, en suivant le corps de doctrine et d'opinion adopté dans les traditions et les usages de la société envisagée. Il va de soi d'autre part que pour donner une description exhaustive et adéquate de l'ensemble il convient d'y suppléer par une description des autres niveaux, présentant des rapports avec le niveau immédiat. Il ne faut pas croire d'ailleurs que ce soit un niveau physique et rien de plus. Il faudra sans doute envisager également une étude des conditions socio-biologiques et du mécanisme psycho-physiologique qui, grace à des dispositions naturelles et à des habitudes acquises, valables pour les expériences sensorielles et autres, permettent aux sujets parlants, appartenant à la communauté linguistique dont il s'agit, de créer, de reproduire, d'évoquer et de manier de diverses façons les éléments d'appréciation dont nous venons de parler, et les unités qui peuvent être en formées.

Ceci nous permet de revenir sur nos pas pour réexaminer de ce pont de vue la substance de l'expression. La question se pose de savoir si on est autorisé à la concevoir comme étant en principe soumise au même régime. Nous croyons pouvoir répondre par l'affirmative. Il suffit en effet de rappeler ce qui a été dit plus haut en parlant de la substance de l'expression. Il est évident que ici encore on est en présence d'un niveau physique et d'un niveau socio-biologique qui sont de tous points comparables à ceux de la substance du contenu; la définition que nous venons de donner du niveau socio-biologique de la substance du contenu s'applique en effet telle quelle, et sans modification aucune, à la substance de l'expression. Il n'est pas moins évident qu'il y a lieu d'ajouter à la description de ces deux niveaux celle d'un troisième, celle que nous avons appelée plus haut la description auditive, ou, plus généralement, la description par apperception, qui s'assimile bien à la description des appréciations collectives que nous avons envisagée pour la substance du contenu.

Concluons donc, du moins provisoirement, que toute substance sémiotique, ou $\Lambda * g^{o}$ (considérée constamment à l'intérieur d'un seul plan) comporte plusieurs niveaux, entre lesquels il y a bien entendu des fonctions définies et un ordre hiérarchique. Il paraît que le niveau qui est en tête de cet ordre hiérarchique est le niveau d'appréciation collective qui, en conséquence de cette circonstance, peut être considéré comme la substance par excellence, la seule substance (dans le sens plus étroit de ce terme) qui du point de vue sémiotique soit immédiatement pertinente. On peut exprimer ce fait dans la terminologie fonctionnelle en disant

que je reconnais le lion et Maître Renard en les rencontrant au jardin d'acclimatation ou dans la forêt. Le renard et le lion ne me viennent pas d'un manuel, ils proviennent de la Bible, des fables, des poèmes, et ce serait pour la linguistique une tâche naturelle et fascinante d'élucider cet arbre généalogique» (*Sprogets musik*, Copenhague 1943, p. 18).

que ce niveau, ou substance sémiotique immédiate, qui évidemment sélectionne la forme qu'elle manifeste, et avec laquelle elle est complémentaire, est à son tour sélectionné par les autres niveaux, parmi lesquels nous avons envisagé deux: le niveau physique et le niveau socio-biologique. Ces deux derniers niveaux sont d'ailleurs spécifiés également par la substance sémiotique immédiate. Ajoutons en outre, pour essayer d'être complet, que relation et corrélation semblent être orientées en sens inverse pour ce qui est des rapports entre le niveau physique et le niveau socio-biologique: le niveau physique semble sélectionner le niveau socio-biologique, et le niveau socio-biologique semble spécifier le niveau physique. Pour résumer, si un niveau est désigné par la formule $\Lambda * g^{\circ}$ munie d'un nombre-indice selon l'ordre hiérarchique des sélections, on obtient:

$\Lambda * g^{\circ}_1$ = niveau d'appréciations collectives (ou substance sémiotique immédiate)

$\Lambda * g^{\circ}_2$ = niveau socio-biologique

$\Lambda * g^{\circ}_3$ = niveau physique

$$* g^{\circ} \leftarrow \Lambda * g^{\circ}_3 \begin{cases} \leftarrow \Lambda * g^{\circ}_2 \\ \leftarrow \Lambda * g^{\circ}_3 \end{cases}$$

$$* g^{\circ} \dashv \Lambda * g^{\circ}_1 \begin{cases} \vdash \Lambda * g^{\circ}_2 \\ \vdash \Lambda * g^{\circ}_3 \end{cases}$$

Si cette conclusion ne peut être donnée qu'avec réserves et à titre d'hypothèse, la principale raison est que, à l'état actuel des recherches, le niveau d'appréciations collectives constitue, surtout dans le plan de l'expression et tout particulièrement pour la substance phonique, un objet mal étudié. Il est vrai que ces derniers temps les faits auditifs ont revendiqué en partie l'estime qui leur revient, mais ils sont à notre avis trop souvent confondus avec les faits physiques, ou acoustiques dans le sens propre. Bien que, en supposant le bien-fondé de notre hypothèse, la description par apperception, et surtout (vue la suprématie traditionnelle de la phonologie sur l'étude des autres substances de l'expression) la description auditive des sons du langage, soit le plus grand *desideratum* de la linguistique actuelle, les contour d'une telle discipline ne se dessinent encore que vaguement. Il paraît évident que, puisqu'il s'agit de dégager des appréciations relativement naïves, et dont le seul fondement «théorique» est à chercher dans ce que nous avons appelé le «corps de doctrine» adopté dans les opinions de la société, la «méta-langue» dont une telle discipline pourrait se servir pour atteindre ses buts ne saurait être que le langage de tous les jours. Les termes techniques de cette méta-langue seraient à puiser surtout dans certains adjectifs qualificatifs trouvés dans la langue même qu'on étudie (on est donc en présence d'un de ces cas où la méta-langue est identique, totalement ou en partie, à la langue-objet): risquons au hasard quelques exemples relativement probables tels que, pour le français, 'clair' : 'sombre', 'fort' : 'faible', 'long' : 'bref', 'haut' : 'bas', 'pesant' : 'léger',

etc. Quoiqu'il en soit, une telle étude demande comme préparatif indispensable un examen du système d'adjectifs de la langue considérée et du substance de contenu qu'ils comportent. Elle demanderait en outre tout un nombre d'autres préparatifs qui, à l'état actuel des recherches, ne sont point, il faut l'avouer, à la portée immédiate du chercheur. Il faut y compter bien des faits d'ordre psychologique, y compris, entre autres choses, ceux de la synesthésie.

Chose curieuse, la linguistique, qui s'était mise en garde bien longtemps contre toute teinte de «psychologisme», semble ici, ne serait-ce que dans une certaine mesure et les proportions bien gardées, être de retour à l'«image acoustique» de F. de Saussure, et également au «concept», à condition d'interpréter ce mot en stricte conformité avec la doctrine que nous venous d'exposer, bref, reconnaître, bien qu'avec les réserves qu'il faut, que des deux côtés du signe linguistique on est en présence d'un «phénomène entièrement psychique»[15]. Mais c'est plutôt une partielle coïncidence de nomenclatures qu'une analogie réelle. Les termes introduits par F. de Saussure, et les interprétations données dans le *Cours*, ont été abandonnés parce qu'ils prêtent à l'équivoque, et il convient de ne pas refaire les erreurs. D'ailleurs nous hésitons pour notre part devant la question de savoir dans quelle mesure les recherches que nous avons ici préconisées peuvent être considérées comme étant d'ordre psychologique; la raison est que la psychologie paraît être une discipline dont la définition laisse encore considérablement à désirer.

Ajoutons brièvement que l'examen ici envisagé ne se confond pas avec la psychologie linguistique dans le sens vague de l'étude du «sentiment linguistique», ni avec l'étude de l'esthétique du langage, ni d'ailleurs avec celle de la correction linguistique—autant de sujets qui sont liés ensemble mais qui constituent un domaine tout à fait à part.

Somme toute, le fait qu'une seule et même forme ou $*g^o$ peut, à l'intérieur de chaque plan pris à part, être manifestée par deux ou plusieurs substances, ne se confond pas avec cet autre fait, bien différent, que chacune des substances comporte plusieurs niveaux (nous les avons prévus en nombre de trois) dont l'un est en tête, puisque sélectionné par les autres, et, par suite de cette circonstance, peut se définir comme la substance sémiotique immédiate. Substance et niveau ne se confondent pas. Il paraît que les niveaux constituent, sans égard à la substance considérée, un système universel (pour lequel il faut prévoir, naturellement, des lacunes possibles dans les réalisations concrètes), ce qui n'est pas le cas des substances. Il y a d'ailleurs plusiers autres circonstances, que l'on trouvera dans la suite, qui induisent à penser que les niveaux se comportent entre eux autrement que les substances entre elles, et—on peut l'ajouter—que ni les substances ni les niveaux ne constituent des strata différents: c'est au contraire l'ensemble de la catégorie composée des diverses substances (chacune organisée dans un système de niveaux) appartenant à une seule et même forme ou $*g^o$ relevant d'un seul plan, qui constitue, par rapport à cette forme, un seul stratum, bien que diversifié de la façon indiqué.

Ceci encore est forcément hypothétique. Nous voudrions d'abord appeler

[15] *Cours de linguistique générale*, p. 28 (nous citons la 2e éd.).

l'attention sur certains faits, gagnés il est vrai par une induction amplifiante, fondée sur la simple expérience des sémiotiques jusqu'ici observées, et dont la plupart n'ont pas, à l'état actuel de nos connaissances, été examinées à fond. Ces faits concernent la nature des relations intrinsèques qui s'observent à l'intérieur de chaque stratum. La nature de ces relations intrinsèques semble être spécifique à la forme et à la substance respectivement, et susceptible par conséquent de définir une différence entre forme et substance à l'intérieur de chaque plan, tandis que, au contraire, les diverses substances et les divers niveaux semblent suivre à cet égard les mêmes règles. C'est tout au moins un indice heuristique pour ne pas confondre la différence entre substances et celle entre niveaux avec celle entre strata.

En combinant les vues traditionnelles sur la délimitation de la linguistique avec l'analyse fonctionnelle entreprise par la glossématique, tout le monde sera d'accord pour reconnaître que le domaine dévolu à la linguistique, le domaine qui lui est réservé et qui constitue son objet propre et spécifique, est constitué par l'ensemble des unités dont la plus large est la phrase[16] et dont la plus petite est le glossème. Il est vrai d'autre part que cette ligne de démarcation ne doit avoir rien d'absolu, et que par contre le point de vue élargi introduit par la glossématique et la nécessité d'une description exhaustive exigent non seulement que dans la description la hiérarchie de la forme, épuisée par l'établissement des glossèmes, soit accompagnée, s'il y a lieu, des hiérarchies des substances, mais aussi que l'analyse circonscrite par la ligne de démarcation indiquée soit précédée d'une analyse dégageant les unités plus larges (telles que, p. ex., productions littéraires, œuvres, chapitres, paragraphes; prémisses et conclusions, etc.). C'est ainsi que la sémiotique réclame le droit d'une discipline qui puisse servir de norme pour toutes les sciences de l'homme[17]. Il n'en reste pas moins que, à condition de sauvegarder une collaboration utile, une division du travail s'impose. Or il paraît que la division traditionnelle (entre 1° sémiotique propre, à laquelle incombe le devoir spécifique d'accomplir l'analyse allant depuis la phrase jusqu'au glossème, 2° les disciplines de substances, telle que phonématique, graphématique, sématique, et 3° autres disciplines telles que l'étude de la littérature, logique, etc.) reflète bien une certaine différence dans la nature des relations intrinsèques caractéristiques des trois domaines.

L'expérience fait voir que les relations contractées par les catégories d'unités larges (réléguées par la tradition à l'étude de la littérature, à la logique, etc.) sont normalement des réciprocités (et le plus souvent des combinaisons; on remarquera bien que nous parlons des relations mutuelles contractées par les seules catégories, et non des relations entre leurs membres, qui peuvent bien être autres[18]), tandis que les sélections entre catégories se rencontrent en sémiotique propre. Dans la procédure d'analyse on peut même fixer un stade où les

[16] Plus exactement: la *lexie* (*Selskab for nordisk filologi* [Copenhague], *Årsberetning for 1948-49-50*, p. 13).

[17] Cf. *Prolegomena*, p. 63 et 69.

[18] *Prolegomena*, p. 55.

sélections entre catégories se rencontrent pour la première fois, et l'expérience montre que ce stade coïncide si souvent avec ce qui est considéré comme le début de l'analyse sémiotique propre que l'apparition de la sélection entre catégories peut être utilisée comme critérium. Deux bases de division[19] se succèdent par conséquent au cours de l'analyse: une première, réciprocité syntagmatique, pour les unités de grande étendue, et une deuxième, sélection, pour les unités sémiotiques propres, et qui est en vigueur depuis la première apparition d'une sélection entre catégories jusqu'au stade où se dégagent les taxèmes. Il va de soi d'ailleurs qu'on ne peut pas indiquer par une règle universelle quelles sont les réalités qui en chaque cas correspondent aux unités dégagés aux stades mentionnés. Le taxème est défini simplement comme l'élément dégagé au stade où l'analyse par sélection est épuisée; les taxèmes sont après coup analysés en glossèmes dont les catégories sont par définition mutuellement solidaires[20]. Souvent, mais pas nécessairement, les taxèmes sont, dans la substance phonique, reflétés par les phonématèmes (y compris les phonèmes). De la même façon on peut donner à l'unité qui la première admet une analyse par sélection une dénomination arbitraire: on l'appellera «lexie»[21]. Souvent, mais pas nécessairement, elle correspondra à la phrase, dans tous les cas où celle-ci admet une analyse en principale (c.-à-d. sélectionnée) et subordonnée (c.-à-d. sélectionnante).

Dans le domains de la substance au contraire, les connaissances dont nous disposons actuellement invitent à penser que les relations entre catégories sont, du moins aux stades initiaux de l'analyse, constamment des solidarités. Ceci semble être le cas pour toute substance et en même temps pour tous les niveaux.

Considérons comme un premier exemple le niveau socio-biologique de la substance phonique, c'est-à-dire la «physiologie des sons», ou phonologie articulatoire. Ici toute unité, chaque son, doit être caractérisé par rapport à un répertoire de catégories (ou dimensions si l'on veut) dont chacune reflète un système sublogique[22] dont les pôles sont par exemple

sonore:sourd
nasal:oral
arrondi:non-arrondi
latéral:non-latéral etc. etc.

De même, dans le niveau des appréciations de la substance phonique, c'est-à-dire dans le niveau auditif, chaque son doit être caractérisé par rapport à un répertoire de catégories dont nous avons essayé de donner plus haut quelques exemples. Dans le niveau physique (acoustique au sens propre), la situation est en principle le même. Il est d'ailleurs moins facile de donner de bons exemples pour ces niveaux, puisque la terminologie laisse encore à désirer et est souvent à cheval sur les deux niveaux.

[19] V. *Prolegomena*, p. 54 sv.
[20] *Prolegomena*, p. 89.
[21] Voir note 16.
[22] L'auteur, *La catégorie des cas* I (*Acta Jutlandica* VII 1, Aarhus 1935), p. 127.

Nous supposons que ce principe vaut pour toute substance et pour tout niveau, et qu'on est partout en présence de catégories mutuellement solidaires, si bien que chaque unité doit être caractérisée par rapport à toutes ces catégories, et se définit comme composée d'un élément provenant de chacune d'entre elles: toute catégorie est représentée par un de ses membres dans n'importe quelle unité[23].

La substance semble donc demander une base d'analyse différente de celle exigée par la forme sémiotique propre. Or, malgré ce principe de solidarité des catégories qui leur est commun, les diverses substances peuvent bien présenter des structures très différentes entre elles, et il en est sans doute de même, bien que probablement à un degré moindre, pour une seule et même matière faisant partie de sémiotiques différentes. Nous le considérons en outre comme probable que, à l'intérieur d'une seule et même substance faisant partie d'une seule et même sémiotique, il y a une certaine correspondance de structure interne dans les différents niveaux, de façon à rendre les niveaux connexes mutuellement superposables selon un principe qui reste encore à trouver. Une telle conformité ne se retrouve ni entre les diverses substances, ni entre substance et forme, ni d'ailleurs entre les deux plans γ^o et g^o. Cette considération nous conduit à éviter de classer les niveaux comme des strata.

La distinction opérée entre substance et niveau nous aidera à mieux comprendre le fait de la multiplicité des substances. Cette multiplicité possible est due à un fait curieux concernant le rapport mutuel entre les niveaux: le niveau d'appréciation, ou substance sémiotique immédiate, ne recouvre pas par nécessité le domaine intégral de chacun des autres niveaux; il peut au contraire se concentrer à n'en refléter qu'un secteur choisi, si bien que, une fois fait ce triage, ce secteur choisi est seul projeté sur l'écran du niveau d'appréciation; ce secteur seul est donc pertinent pour la substance dans un tel cas. Il est évident que ce qui constitue la véritable différence entre des substances telles que la substance phonique, la substance graphique, les signaux par pavillons etc. est le choix de secteur des niveaux socio-biologique et physique: il peut s'agir, pour se borner à une indication grossière, de l'ordre acoustique ou de l'ordre visuel, et ces deux

[23] Il va de soi qu'il faut prévoir, ici comme partout, des syncrétismes et des participations, selon le système reconnu par la sémiotique particulière qui fait l'objet de l'analyse. Ainsi un son, dans une langue donnée, n'est pas a priori nécessairement ou sourd ou sonore; il peut être sourd et sonore (que ce soit alternativement ou à force d'un glissement au cours de son émission), et il peut (du moins théoriquement) recevoir la définition 'ni sourd ni sonore', laquelle représente la case neutre de la catégorie; même dans le dernier cas, la catégorie est donc représentée. D'une façon générale, il n'y a jamais *absence* d'une catégorie dans une unité de substance. On voit que notre analyse diffère de celle des adhérents de la théorie des «traits distinctifs». Nous en discuterons autre part les fondements. Ajoutons seulement, pour la clarté du présent exposé, que dans un exemple tel que celui fourni par le *g* russe, nous n'hésitons pas à reconnaître que c'est une consonne orale (ou, tout au moins, à la caractériser positivement par rapport à la catégorie 'nasal' : 'oral'). Omettre, dans la définition phonématique du *g* russe, toute mention de ce fait, serait commettre une fausse analogie d'après les analyses par sélection: on ne procéderait pas en d'autres cas de la même façon. Ainsi, données les unités *pe*, *po*, *to* (mais, au même stade de l'analyse, ni *te*, ni *e*), on peut et doit identifier le *p* de *pe* comme un *p*. De même, en regard de *tr* et *pr*, et de l'absence, au même stade de l'analyse, de *ʃ* et de *pʃ*, on identifierait bien le *t* dans *tʃ*.

secteurs peuvent encore se subdiviser, de façon à n'en faire valoir qu'une partie, comme c'est quelquefois le cas pour les couleurs qui peuvent être pertinentes pour les signaux par pavillons mais non pour les caractères de l'alphabet.

Il s'ensuit qu'une telle délimitation de secteurs dans les deux niveaux inférieurs de la substance est la condition de la multiplicité des substances. Du moment que ce sectionnement manque à se produire, et que la substance couvre le domaine intégral des niveaux inférieurs sans se concentrer sur un secteur particulier, il ne peut y avoir qu'une seule substance.

Ce cas peut surtout être observé pour le plan du contenu des sémiotiques linguistiques, et c'est pourquoi les linguistes ne peuvent reconnaître pour le contenu de ces structures qu'une seule substance. La raison n'est pas loin: c'est qu'une langue est par définition une sémiotique passe-partout, destinée à former n'importe quelle matière, n'importe quel sens, donc une sémiotique à laquelle toute autre sémiotique peut être traduite sans que l'inverse soit vrai[24]. Ce caractère intégral de la substance de contenu d'une langue va jusqu'à inclure dans cette substance celle de l'expression, et d'ailleurs aussi les formes de la même langue, ce qui est la condition nécessaire pour pouvoir utiliser la langue comme la méta-langue dont on se sert pour la décrire. La corrélation entre la substance du contenu et celles de l'expression est donc, dans le cas d'une sémiotique linguistique, une participation unilatérale complète, selon la formula α:A[25].

Dans les sémiotiques non-linguistiques, au contraire, les niveaux sont toujours représentés par des secteurs, et une multiplicité de substances du contenu est par conséquent possible: une même forme du contenu admet diverses «interprétations».

On peut ajouter que l'on peut prévoir également une structure où le domaine de la substance de l'expression occupe un domaine illimité. Ce cas se vérifie en effet pour les sémiotiques connotatives dont le plan d'expression est constitué par une sémiotique linguistique.

Notre thèse finale, qui va nous occuper maintenant, et à laquelle il a d'ailleurs été fait allusion déjà au début du présent article, concerne une série d'analogies frappantes qui règnent pour chacune des paires suivantes de strata: 1° substance du contenu et forme du contenu, 2° forme du contenu et forme de l'expression, 3° forme de l'expression et substance de l'expression. On peut en effet établir certaines lois qui dirigent les relations entre strata (manifestation d'une part, relation sémiotique ou dénotation, de l'autre), ou plutôt les rapports qui existent entre les relations entre strata (relations «interstratiques» si on veut) d'une part et, de l'autre, les relations qui valent à l'intérieur d'un seul et même stratum (et que nous appellerons relations *intrinsèques*).

A l'intérieur de chacune de ces paires, les relations interstratiques consistent en une projection de certaines unités d'un stratum sur l'autre, et inversement. Grâce à cette projection surgissent, dans chaque stratum, outre les unités intrinsèques certaines autres, définies non par les relations reconnues par ce stratum même, mais par la projection, donc des unités qui, du point de vue du stratum

[24] V. *Prolegomena*, p. 70.

[25] Voir pour ces symboles surtout *La catégorie des cas* I, p. 113.

considéré, sont étrangères, intruses, imposées à ce stratum par un autre; nous les appellerons unités *extrinsèques*.

Considérons d'abord la paire centrale, celle qui est constituée par γ^o et g^o.

Dans chaque stratum de cette paire, les unités extrinsèques sont les deux faces du signe, liées entre elles par la relation sémiotique (ou, dans le cas d'une langue, la dénotation); nous proposons de les appeler *glossématies*, et de désigner une glossématie par la formulae $*g\frown$. Dans γ^o, ces unités extrinsèques sont les «contenus de signes» ou *plérématies* ($\gamma\frown$), imposées au plan du contenu par le plan de l'expression; dans g^o, les unités extrinsèques sont les «expressions de signes» ou *cénématies* ($g\frown$), imposées au plan de l'expression par le plan du contenu.

A propos de ces unités nous avons quatre observations à faire.

1°.—Les unités en question, ou glossématies, sont en principe toujours des *variantes*.

Tout le monde sera d'accord pour reconnaître que c'est souvent le cas, puisque c'est le cas banal des homonymes et des synonymes. Dans ce cas on dit que les glossématies en question sont des variantes d'une même *glossie* (les plérématies seront les variantes d'une même *plérie*, et les cénématies seront les variantes d'une même *cénie*; la formula employée pour indiquer une glossie sera) (, c.-à-d. des parenthèses renversées entre lesquelles sont mises les variantes, ou glossématies, qui sont membres de la glossie en question).

Ainsi, le fait que, en français écrit, $g\frown$ *-s* sert à exprimer le pluriel de noms et, dans le verbe, la deuxième personne du singulier (*parle-s*) ou un syncrétisme de la première et de la deuxième personne du singulier (*fini-s*, *vend-s*) constitue une homonymie qui produit la plérie

)'plur.', '2e p. sg.', '1re/2e p. sg.'(

De même, puisque $g\frown$ *-e* exprime le féminin d'adjectifs et le singulier du présent du subjonctif[26] (*vend-e*, *-e-s*) ou d'un syncrétisme de l'indicatif et du subjonctif (*parl-e*, *-e-s*), on obtient la plérie

)'fém.', 'subj. prés. sg.', 'ind./subj. prés. sg.'(

Inversement, les synonymies produisent des cénies, comme p. ex. celle constituée par les désinences du participe passé et par les racines du verbe *aller:*

)*-é*, *-i*, *-u*(

)*all-*, *ir-*, *v-*(

Mais il ne faut que peu de réflexion pour reconnaître que ce que la tradition nous décrit comme des homonymies et des synonymies ne constitue qu'un cas particulier d'un phénomène plus répandu, et qu'il n'y a qu'une différence de degré entre ces cas extrêmes et une foule d'autres où il est également facile de voir la différence des variantes. C'est aussi pourquoi l'homonymie et la synonymie

[26] On fait abstraction de l'imparfait du subjonctif pour éviter les complications inutiles.

restent dans la linguistique traditionnelle mal définies, et que la tradition tend souvent à élargir considérablement la valeur de ces termes. Il suffit sans doute de rappeler brièvement que les diverses significations d'un même mot nécessiteront sans doute dans bon nombre de cas des définitions partiellement différentes de la forme plérématique; c'est ainsi que, en français, les mots *plume* ou *fille* constituent sans doute des pléries relativement complexes[27], et que la forme phonématique du mot français «*plus*» permet d'établir une cénie

$$)plyz,\ ply,\ pyz,\ py,\ plys($$

ou que les diverses formes phonématiques prises par le thème d'adjectif «*grand-*» (devant voyelle initiale, devant consonne, devant *-e* féminin) constituent également une cénie comprenant plusieurs membres. En effet, à y regarder de plus près, les exemples foisonnent, et il convient de reconnaître que les glossématies sont en principe toujours des variantes.

2°.—Les glossématies sont *arbitraires*. Ce fait est trop connu pour qu'il y ait lieu d'y insister longuement. En effet, ni au point de vue interstratique ni au point de vue intrinsèque, il n'y a rien d'inhérent qui motive la relation sémiotique particulière pour chaque signe ni la forme particulière prise par la glossématie dans chaque cas donné. C'est aussi pourquoi les homonymies et les synonymies ont ce caractère fortuit, réunissant dans une même catégorie (glossie) des membres absolument disparates et hétérogènes. En outre, sauf quelques cas qui constituent plutôt une exception qu'une règle (langues monosyllabiques par exemple), les glossématies ne coïncident pas avec des unités intrinsèques; les étendues des glossématies sont arbitraires et diverses.

3°.—La troisième observation n'est qu'une conséquence et une explication de celles qui précèdent: la relation interstratique (en l'espèce, la relation sémiotique) relève de l'*usage*. Puisqu'elle ne présente en principe aucun rapport avec les fonctions intrinsèques, la relation interstratique n'a pas d'emprise sur le schéma[28], si bien que les signes d'une langue peuvent changer du tout au tout sans que la structure interne de la langue en soit affectée.

4°.—Les unités intrinsèques dont une glossématie se compose peuvent être appelées *figures*[29]; les figures minimales sont les taxèmes; une glossématie peut être bâtie d'un seul ou de plusieurs figures. Mais il ne faut pas penser que les figures se dégagent par une analyse du signe: elles se dégagent au contraire uniquement par une analyse des unités intrinsèques. Le caractère arbitraire de la glossématie, et particulièrement de son étendue syntagmatique, suffit pour le faire voir. Or, ce qui nous semble ici particulièrement intéressant, c'est que *les unités intrinsèques dont on tire les figures peuvent être d'une étendue syntagmatique plus grande qu'une glossématie dans laquelle elles entrent*. Ainsi, les figures que l'on reconnaît, en français, dans $g_f \frown al$-, $g_f \frown ir$- et $g_f \frown v$- (formes phonématiques prises par la cénie du verbe «*aller*») sont gagnées et définies à la base de la (pseudo-)syllabe, et de préférence de la (pseudo-)syllable d'étendue maximale.

[27] J. Vendryes, *Le langage*, p. 208.
[28] Sur *schéma* et *usage* voir *Langue et parole* (cité plus haut, note 6).
[29] V. *Prolegomena*, p. 29.

(Remarquer aussi, d'ailleurs, que dans $g_{f}\frown al$- et $g\frown_{f} ir$- la frontière syllabique sépare les deux figures.)

Passons maintenant aux deux paires marginals, celles qui sont constituées par forme et substance, ou, plus exactement, par γ^{o} et $\Lambda\gamma^{o}$ et par g^{o} et Λg^{o}, respectivement. On peut faire à propos de ces paires, et des unités dont elles consistent, exactement les mêmes observations que pour la paire centrale: sur bien des points essentiels, la manifestation, ou relation entre forme et substance à l'intérieur d'un plan, se comporte exactement comme la relation sémiotique ou dénotation. On n'a d'ailleurs guère besoin d'insister longuement sur les traits caractéristiques qui se retrouvent dans les deux paires marginales et qui viennent d'être décrits pour la paire centrale: la plupart d'entre eux sont bien connus. Ainsi, il est évident que

1° les unités extrinsèques sont, ici encore, des *variantes*[30];

2° les unités extrinsèques sont *arbitraires*, tout comme le choix de substance est, par rapport à une forme donnée, arbitraire;

3° la relation interstratique, en l'espèce, la manifestation, relève de l'*usage*.

Sur ces trois points il y donc une analogie très nette entre les deux paires marginales et la paire centrale, à condition évidemment de tenir compte des différences, énumérées plus haut, entre les deux types de paires, et dont la plus décisive est celle que la relation sémiotique est une solidarité tandis que la manifestation est une sélection.

Le quatrième point encore est un point d'analogie, et le seul point qui nous retiendra:

4°.—Pour disposer d'une terminologie relativement simple (correspondant à l'emploi des termes *glossématie*, *plérématie*, *cénématie* pour les variantes d'unités extrinsèques dans le domaine de la paire centrale de strata) nous proposons, pour le domaine des paires maginales de strata, d'appeler une variante d'une unité extrinsèque un *terme de manifestation*; dans la forme, un terme de manifestation sera appelé une *manifestée*; dans la substance, un terme de manifestation sera appelé une *manifestante*. Encore, vue l'analogie que nous allons décrire, nous proposons d'élargir l'emploi du terme *figure* de façon à le rendre utilisable aussi pour l'étude des rapports entre forme et substance. Donc, par *figures* on comprendra, dans la forme et dans la substance respectivement, les unités intrinsèques dont un terme de manifestation se compose. Ici encore, un terme de manifestation peut être d'un seul ou de plusieurs figures. Un exemple d'une manifestante consistant en une seule figure est la grandeur phonique [˜] en français qui a été citée plus haut: [˜] manifeste un taxème de l'expression (*n*, en l'espèce) et est par conséquent une manifestante; elle ne consiste qu'en une seule figure de la substance phonique, à savoir la nasalité.

Ici encore on peut montrer que les unités intrinsèques dont on tire les figures peuvent être d'une étendue syntagmatique plus grande qu'un terme de manifestation dans lequel elles entrent. A l'état actuel de nos connaissancss, il sera sans doute le plus facile de démontrer ce fait en puisant les exemples dans la manifestation phonématique. Supposons que «*bec de gaz*» est prononcé avec un

[30] Sur la conception des variantes voir *Prolegomena*, p. 52 sv.

k sonore (que nous nous permettons ici d'écrire [g]) devant *d*; l'unité sonore [-gd-] est une grandeur phonique qui est reflétée dans la forme par la manifestée correspondante, laquelle est *-k(ə)d-*, répartie sur une suite de (pseudo)syllabes, et décomposable en des taxèmes, ou éléments intrinsèques de la forme, qui ne se dégagent que de l'ensemble de cette suite. De même, la vocoïde nasale du mot français «*bon*» a pour manifestée correspondante une unité composée d'une voyelle + *n*, ce qui n'est démontrable qu'en comparant avec les cas où ces deux taxèmes sont répartis sur deux (pseudo)syllabes. La nasale vélaire de quelques langues, telles que le danois, a pour manifestée, du moins dans quelques cas, le groupe *ng*, à condition que ce groupe soit homosyllabique; il ne se décompose par conséquent en taxèmes qu'en partant de syllabes entières et même de groupes de syllabes. Ces exemples, qui servent à montrer la règle pour les manifestées, se multiplient facilement. Il ne serait pas moins facile d'en donner pour les manifestantes. Ainsi, le taxème *m*, dans «*mère*» par exemple, a pour manifestante le son [m]. Ce son peut être décomposé en figures phoniques, telles que 'nasalité', 'sonorité', 'occlusion buccale', etc. Or, ces figures sont tirés d'unités intrinsèques d'ordre phonique qui sont d'une plus grande étendue que le son même [m]; ainsi, la nasalité se trouve à travers le mot entier «*maman*» [mãmã], qui par ce fait est en contraste avec le mot «*baba*»; la sonorité se trouve sur tout le parcours d'un énoncé tel que «*je vous aime bien, ma mère adorée*».

Cette dernière observation nous semble présenter un intérêt particulier pour la méthode à employer dans l'analyse. La décomposition des unités intrinsèques de substance en des figures (définies, bien entendu, par des fonctions intrinsèques) peut souvent être rapportée à une opération qui dans la procédure est de beaucoup antérieure à celle où l'analyse de la forme permet de dégager les taxèmes. C'est dire que la bifurcation entre forme et substance commence à un moment plus avancé de l'analyse qu'on ne le croit souvent, et que les deux hiérarchies se séparent déjà à ce stade. Dès le moment même où l'on rencontre une différence telle que celle entre «*baba*» et «*maman*», on est devant un choix: ou bien il faut procéder à diviser ces unités en des (pseudo)syllabes, (pseudo)-voyelles, (pseudo)consonnes, ou bien on les analyse d'emblée en nasalité et autres qualités phoniques. En vérité ce n'est pas un choix, mais une obligation de suivre les deux voies, mais évidemment—puisqu'il y a conflit entre elles—de les suivre séparément. Ce critérium pour la distinction entre forme et substance sémiotiques semble précieux.

D'une façon plus générale, les quatre observations que nous avons pu faire sur les analogies entre les paires de strata semblent constituer un critérium utile pour déterminer avec plus de rigueur et d'exactitude les frontières entre strata. Nous croyons aussi qu'elles peuvent être utilisés négativement, et qu'elles peuvent servir à montrer, entre autres choses, que les diverses substances qui peuvent se trouver à l'intérieur d'un même plan, et les divers niveaux ne sont pas des strata différents. Ici encore, nous regardons la quatrième observation comme particulièrement décisive.

Au cours de la dernière partie de notre exposé on a eu l'occasion de considérer les faits de l'*usage*. Nous croyons en effet que les réflexions que nous avons

maintenant terminées permettent de donner une définition non seulement de l'*usage*, mais aussi, plus généralement, de la *parole*, et, d'autre part, plus particulièrement, de la *norme*, dans la mesure où ce terme d'abstraction se montrerait utile. Nous soumettons ces définitions au lecteur:

On peut définir la *parole* par la rencontre même et l'entrecroisement des strata. La parole en effet est, en dernière analyse, tout ce qui est arbitraire dans le langage. La parole se définit comme l'ensemble des relations interstratiques effectivement exécutées.

L'*usage*, à son tour, est évidemment ce qu'il y a de stabilisé dans la parole. L'usage se définit comme l'ensemble des connexions[31] interstratiques effectivement exécutées.—Les combinaisons qui sont des variantes des connexions interstratiques appartiennent à la parole sans appartenir à l'usage. Elles constituent, en d'autres termes, ce qui reste de la parole en soustrayant l'usage. C'est ce qu'on appelle l'*acte* linguistique ou sémiotique.

A la différence de l'usage, la *norme* doit être l'ensemble des relations interstratiques possibles.

Le *schéma* sémiotique (et linguistique) par contre est en dehors de cet ordre d'idées: ce terme ne se rapporte qu'aux fonctions intrisèques dans la forme de chacun des plans pris à part.

Nous espérons que l'étude qu'on vient de lire pourra contribuer, du moins provisoirement, à parer à certains malentendus et certaines confusions qui, sauf erreur, se trouvent encore dans ce domaine qui, à y regarder de plus près, s'est trouvé être encore pratiquement vierge. Les résultats que nous croyons avoir obtenus laissent sans doute encore à désirer, et demanderont des précisions ultérieures, des corrections et des remaniements. La première condition d'un tel progrès est qu'on se serve de notions aussi bien définies que possible. On pourra, si on le désire, se complaire à bâtir des allégories plus ou moins spirituelles, en prétendant par exemple que la substance graphique 'exprime' la substance phonique, donc considérer ces deux substances comme des 'plans' du second degré, ou bien que les niveaux inférieurs seraient des 'substances' du second degré par rapport à la substance sémiotique immédiate considérée comme 'forme' du second degré. Mais nous avons justement voulu prémunir contre cet abus de métaphores et d'une terminologie qui porte encore l'empreinte de ce «corps de doctrine» qui constitue le niveau supérieur de la substance du contenu du langage, mais dont la science doit s'affranchir.

Université de Copenhague

[31] Ou *cohésions* (ainsi dans *Prolegomena*).

LES PROBLÈMES DES LOIS INTERNES DE DÉVELOPPEMENT DU LANGAGE ET LA LINGUISTIQUE SOVIÉTIQUE

R. L'HERMITTE

On a pu trouver, dans les travaux des linguistes soviétiques, au cours des dernières années, des références fréquentes à la notion de lois internes de développement du langage. L'importance attachée à cette notion ressort du fait que des débats réunissant un grand nombre de participants lui furent consacrés.

Le premier se tint à Léningrad, du 20 au 22 Janvier 1952; il s'agissait d'une session de l'Institut de Linguistique de l'Académie des Sciences de l'U.R.S.S., à laquelle avaient été invités des spécialistes de linguistique et de pédagogie habitant la seconde ville soviétique.

Le second se déroula du 4 au 7 Février 1952 à Moscou. Organisé par le Conseil scientifique de l'Institut de linguistique, il réunit plus de 1000 participants.[1] Le président de l'Institut de Linguistique, V. V. Vinogradov, y présenta un long rapport auquel nous nous réfèrerons souvent dans ce travail.[2] V. V. Vinogradov tentait d'y définir la notion de lois internes de développement du langage d'une part en se fondant sur les conceptions théoriques fondamentales admises en U.R.S.S., et, d'autre part, en utilisant un matériel linguistique essentiellement relatif au russe.

Ce rapport, les débats du colloque de Moscou, les thèses qui y furent adoptées, les articles ou travaux de différents linguistes de l'U.R.S.S. permettent de dégager, dans ses grandes lignes, la conception qu'on se fait en Union Soviétique d'une notion aussi importante. Il semble que, jusqu'à présent, aucune étude d'ensemble n'ait été faite mettant en évidence l'action de ces lois tout au long de l'histoire d'une langue donnée; toutefois les éléments déjà fournis permettent, dès aujourd'hui, une présentation de cet important problème.

Une première question se pose. Comment la notion de lois internes de développement du langage est-elle apparue dans la linguistique soviétique? Il faut, pour le comprendre, se rappeler le rôle déterminant joué en U.R.S.S., pendant de longues années, par N. Ja. Marr et ses disciples, et les importants débats publics de 1950 qui aboutirent à un rejet des thèses fondamentales de ces linguistes.

En effet, c'est en opposition à la conception marriste qui voyait dans la naissance de nouveaux phénomènes linguistiques le simple reflet, la conséquence directe de phénomènes sociaux, qu'on a été amené en U.R.S.S. à souligner la spécificité du langage et, partant, la spécificité de ses développements et d'un grand nombre de ses innovations.[3] Mais en insistant sur l'existence et l'action de ces lois internes, les linguistes soviétiques entendent également réagir contre

[1] C.-r. dans *Voprosy jazykoznanija*, 3(1952).141ff.

[2] V. V. Vinogradov, Ponjatije vnutrennikh zakonov razvitija jazyka, *Voprosy jazykoznanija*, 2(1952).3ff.

[3] V. V. Vinogradov, *op. cit.* 6.

certaines conceptions d'un grand nombre de linguistes contemporains. Ils s'élèvent en particulier contre tous ceux qui nient l'existence de lois en linguistique.[4] De même ils entendent s'opposer à ceux qui, tout en reconnaissant l'existence de certaines tendances régulières dans le développement des langues, posent dans le même temps qu'on ne saurait dégager les lois régissant ce développement.[5]

Avant d'examiner l'idée que se font les linguistes soviétiques des lois internes de développement du langage, il importe de préciser à l'intérieur de quelles limites ils en voient les manifestations et dans quelles mesures ils admettent la spécificité même du langage.

«Par lois internes de développement du langage, écrit le linguiste géorgien A. S. Čikobava,[6] on entend toute la diversité des *formules des changements* du lexique et de la structure grammaticale, du phonétisme et des processus phonétiques, du sens des mots et expressions, changements qui *ne peuvent être ramenés* aux changements intervenus dans la base.» Par base, A. S. Čikobava entend essentiellement la structure sociale. Il ajoute d'ailleurs que «les lois internes de développement du langage n'embrassent naturellement pas les changements dûs à l'influence d'autres langues.»[7] Une formulation aussi catégorique appelle sans doute des nuances: car il est clair que l'influence d'une langue sur une autre ne peut s'exercer que dans le cadre et en combinaison avec les lois internes de développement de cette dernière.

A. S. Čikobava donne d'ailleurs deux exemples qui illustrent clairement sa pensée: l'enrichissement du lexique russe par l'apparition, par ex., des mots: *penitsillin*, *streptotsid*, *traktor*, *kombajn*, *samosval*, est lié directement à des changements intervenus dans l'existence du peuple russe: à savoir l'apparition des objets correspondants dans son existence quotidienne. Il est évident par contre que, p. ex., ne sont dûs ni au développement du commerce ou de l'artisanat, au déplacement du centre de la vie politique vers Vladimir, ni à tout autre événement de l'histoire de la société russe, l'existence de l'akanie, la disparition, en russe, de l'imparfait et de l'aoriste. Ces derniers phénomènes relèvent donc de l'action des lois internes de développement du langage.[8]

La notion de lois internes de développement du langage étant ainsi précisée, une seconde question se pose à son tour: Existe-t-il de telles lois pour le langage en général ou seulement pour chaque langue en particulier?

Avec I. V. Staline,[9] les linguistes soviétiques estiment que les deux lois suivantes régissent le développement de toute langue.

I.—Loi du passage d'une qualité à une autre par l'accumulation lente et progressive des éléments de la nouvelle qualité, de la nouvelle structure, et par le dépérissement parallèle des éléments anciens.[10] (Précisons que l'affirmation de

[4] V. V. Vinogradov, *op. cit.* 4–5.
[5] V. V. Vinogradov, *op. cit.* 39.
[6] A. S. Čikobava, *Vvedenije v jazykoznanije*, 81.
[7] A. S. Čikobava, *ibid.*
[8] A. S. Čikobava, *op. cit.* 82–83.
[9] I. V. Stalin, *Marksism i voprosy jazykoznanija*, 28.
[10] V. V. Vinogradov, *op. cit.* 31.

cette loi s'inscrivait en réaction contre la notion de transformations radicales, par bonds, entre un état de langue donné et un autre—notion propre aux Marristes.)

II.—Loi de la stabilité remarquable de la base d'une langue. Les linguistes soviétiques entendent par là, d'une part, la structure grammaticale, d'autre part, ce qu'ils appellent le fonds essentiel du vocabulaire par opposition à l'ensemble du lexique.[11]

Notons que s'il semble que V. V. Vinogradov et A. S. Čikobava considèrent ces lois comme des lois générales internes, V. A. Zvegintsev leur refuse ce dernier qualificatif.[12]

Au nombre des lois générales de développement des langues certains auteurs rangent le développement des catégories abstraites à partir des catégories concrètes tant en ce qui concerne la morphologie que le lexique.[13] K. V. Lomtatidze fournit ainsi toute une série d'exemples empruntés à des langues variées pour illustrer cette conception: évolution sémantique de l'allemand *Zweck* (passant de «clou» à «but»), de kabarde *fé* (passant de «peau» à «couleur», «aspect»), de géorgien *vinaidan* (passant de «d'où, de quel endroit» à «parce que»), élimination dans la formation du futur périphrastique russe des auxiliaires *imu*, *načnu*, *učnu*, au profit de *budu*, plus abstrait, généralisation en géorgien du préfixe des participes *m* après la disparition des classes (*m* était lié à la classe de l'homme): ex. *matarebeli* «train».[14] Il est clair qu'on pourrait aisément multiplier de tels exemples.

Le développement des catégories abstraites amène à se demander s'il n'existerait-il pas—comme l'admet Jespersen—une loi générale du progrès dans le langage? V. A. Zvegintsev s'élève contre une telle conception qui procède d'une confusion entre pensée et langage, entre catégories de la pensée et procédés linguistiques. S'il y a progrès, perfectionnement, dans chaque langue, c'est sur la base des éléments existant dans cette langue, l'enrichissement, le développement des possibilités d'expression de deux langues pouvant se faire selon les procédés les plus différents, sans qu'on puisse faire intervenir un jugement de valeur à leur égard.[15]

Avant d'en venir à l'examen des lois internes de développement d'une langue donnée, voire d'un groupe de langues, il importe de préciser quelques notions qui, comme le révèlent les débats qui se sont institués entre différents linguistes soviétiques, risquent, si elles sont mal entendues, de fausser certaines perspectives.

C'est ainsi que V. V. Vinogradov rappelle que ces lois ne peuvent être éternel-

[11] V. V. Vinogradov, *ibid.* A. S. Čikobava, *op. cit.* 81; V. A. Zvegintsev, Ponjatije vnutrennikh zakonov razvitija jazyka v svete rabot I. V. Stalina po jazykoznaniju, *Voprosy jazykoznanija v svete trudov I. V. Stalina*, 189.

[12] V. A. Zvegintsev, *op. cit.* 201–202.

[13] A. S. Čikobava, *op. cit.* 82.

[14] K. V. Lomtatidze, O zakonomernostjakh istoričeskogo razvitija jazyka, *Voprosy teorii i istorii jazyka v svete trudov I. V. Stalina po jazykoznaniju*, 65–66.

[15] V. A. Zvegintsev, *op. cit.* 202–204.

les. En d'autres termes, il est clair qu'une loi ne peut manifester son existence et son action que si les conditions même de son apparition, si le champ de son application existent, de même qu'en chimie, par exemple, les lois de la combustion ne peuvent jouer que s'il y a de l'oxygène. «Les lois internes de développement d'une langue concrète, ce sont les lois de son dynamisme, de ses changements quantitatifs et qualitatifs, de son passage d'une qualité à une autre. C'est pourquoi elles sont changeantes; de même que les lois internes de développement d'autres phénomènes sociaux, elles varient avec les époques.»[16]

Vinogradov estime également nécessaire de distinguer entre lois internes de développement d'une langue et règles de fonctionnement de cette dernière, celles-ci étant le résultat des premières, même si elles ont cessé d'agir.[17] C'est ainsi, dit-il, qu'en russe les règles relatives à la conjugaison des verbes non productifs (ex.: *gresti–grebu–grjob*, *leč'–ljagu–ljog*, *strič'–strigu–strižeš'*) sont le résultat ou le produit de lois internes de développement de la langue dont l'action a déjà cessé. Il en va de même pour la déclinaison des pronoms.

Par contre, les règles de formation productive de verbes à partir de substantifs ou d'adjectifs, et, parallèlement, les règles relatives à la conjugaison de ces verbes, apparaissent comme le résultat ou le produit de lois dont l'action se poursuit de nos jours. Il en va de même pour les règles concernant le passage de participes au rang d'adjectifs, etc.

Nous avons là quelques exemples de ces lois internes de développement pour le russe. Signalons également les exemples présentés par N. S. Pospelov au colloque de Moscou:[18] les processus de différenciation des types de déclinaison des substantifs selon le genre, la grammaticalisation de la catégorie des numéraux, le développement des catégories d'aspect, de temps et de modes des verbes, etc.

Plusieurs auteurs ont montré que l'existence et l'action de ces lois internes, qui ne peuvent être mises en évidence qu'après une étude historico-comparative minutieuse, pouvaient concerner non seulement une langue donnée, mais un groupe de langues parentes. (Notons que Zvegintsev estime que dans ce cas les éléments originels de ces lois doivent être recherchés dans la langue-base qui est à l'origine de ce groupe de langues.) Pour les langues indo-européennes V. V. Vinogradov signale—pour recherches ultérieures—la tendance à l'unification et à la simplification de la déclinaison sur la base du genre des substantifs, les tendances au développement de types variés de propositions, notamment de propositions conjonctives.[19] A. S. Čikobava fait allusion à la diminution des consonnes palatales et à l'accroissement correspondant des consonnes prépalatales pour les langues ibéro-caucasiques.[20]

Toutefois, nous pouvons remarquer que, comme il a été dit au début de cet

[16] V. V. Vinogradov, *op. cit.* 35.

[17] V. V. Vinogradov, *op. cit.* 36.

[18] Diskussija o vnutrennikh zakonakh razvitija jazyka v Učenom sovete Instituta jazykoznanija A. N. S. S. S. R. *Voprosy jazykoznanija*, 3(1952).143.

[19] V. V. Vinogradov, *op. cit.* 39.

[20] A. S. Čikobava, *op. cit.* 82.

article, l'étude concrète de la mise en évidence et de l'action des lois internes de développement des langues est à peine commencée. Ce que constatent également les linguistes soviétiques qui reconnaissent que s'ils se sont surtout penchés, jusqu'à présent, sur les problèmes théoriques généraux relatifs à cette question, ils n'ont que peu développé les études de détail.

On a parlé jusqu'ici de lois générales, de lois internes propres à un groupe de langues ou à une langue déterminée. Mais n'existerait-il pas des lois particulières pour les différents éléments de la langue: vocabulaire en général, fonds essentiel du lexique, structure grammaticale, phonétisme? C'est l'avis exprimé par P. Ja. Černykh,[21] opinion qui, dans sa rigueur, a suscité un certain nombre d'objections de la part d'autres linguistes soviétiques. C'est ainsi que V. V. Vinogradov proteste contre les séparations tranchées introduites par P. Ja. Černykh entre les différents éléments constitutifs d'une langue; il estime au contraire qu'il s'exerce une constante interaction entre eux. C'est là une idée qui traverse tout le rapport qu'il a présenté au colloque de Moscou. «Dans la structure d'une langue, dit-il notamment, les différentes catégories de phénomènes, qui se trouvent liées d'une manière interne et existent en interrelation (p. ex. les systèmes de déclinaison et de conjugaison, le système des temps, des modes, des aspects et des formes du verbe, etc.) sont semblables à une chaîne dont les maillons sont soudés, liés entre eux, et tiennent étroitement les uns aux autres.»[22] C'est ainsi, souligne-t-il, qu'en présence d'une tendance déterminée, des modifications d'ordre lexical peuvent, dans le cours d'un processus d'abstraction grammaticale ou de généralisation sémantique, entraîner la création de nouveaux éléments, l'apparition d'une nouvelle structure dans une langue donnée. A titre d'exemple, il donne l'incorporation dans le système de formation des aspects en russe, dans la période allant du XII[e] au XVI[e] siècle, de la préfixation verbale qui accélèra un processus dont les racines remontaient très loin, à la période du slave commun.[23] C'est une idée semblable qu'expose V. N. Jartseva[24] dans un article portant essentiellement sur l'histoire de l'anglais. Elle donne comme exemple, entre autres, ce qu'elle appelle la formation «radicale» de nouveaux mots, c'est-à-dire la transformation de substantifs en verbes (ex. *to floor* an adversary, *to savage* an opponent). Il s'agit bien d'un changement de catégorie du mot considéré—et non d'une utilisation syntaxique particulière—puisque l'ancien nom admet dorénavant toutes les catégories grammaticales du verbe (personne, temps, mode, forme). Ce changement apparaît comme lié, historiquement, au renforcement de traits typiques de la structure grammaticale de l'anglais: pauvreté de la flexion, faible degré de variabilité du mot, détermination du rôle syntaxique du mot par sa place dans la proposition. «Nous avons voulu montrer ainsi, écrit-elle, que la

[21] P. Ja. Černykh, O svjazi razvitija jazyka s istorijej naroda, *Izvestija Akademii Nauk S.S.S.R., Otdelenije literatury i jazyka*, 3(1951).247ff.

[22] V. V. Vinogradov, *op. cit.* 37.

[23] V. V. Vinogradov, *op. cit.* 38.

[24] V. N. Jartseva, Razvitije sistemy jazyka, *Voprosy teorii i istorii jazyka v svete trudov I. V. Stalina po jazykoznaniju*, 68ff.

structure grammaticale d'une langue et son fonds lexical essentiel sont étroitement liés et que les lois internes du développement de la structure grammaticale d'une langue donnée ont leur reflet dans le caractère spécifique des processus de développement du fonds lexical essentiel.»[25] Enfin V. A. Zvegintsev, énonçant des conceptions identiques, souligne que, pour une langue donnée, les règles grammaticales s'abstraient à partir d'un matériel lexical déterminé, dont la formation dépend, elle-même, des conditions historiques dans lesquelles se trouve cette langue. Que serait la grammaire, dit-il, si elle s'édifiait en dehors du vocabulaire? Elle apparaîtrait comme le reflet direct—sous forme de règles constantes—des rapports, des liaisons, que l'homme, par son activité intellectuelle, a découvertes au sein de la réalité extérieure. La grammaire se confondrait avec une sorte de logique universelle. Les grammaires des différentes langues devraient pratiquement se ressembler, leurs seules différences résidant dans leur plus ou moins grand degré de perfection logique. Ce n'est évidemment pas le cas. Une des raisons essentielles c'est que la grammaire d'une langue donnée est indissolublement liée au lexique de cette langue dont elle s'est abstraite progressivement.[26]

Il est, d'autre part, assez curieux que les manifestations des lois internes de développement du langage dans le domaine de la phonétique n'aient, jusqu'à présent, que peu retenu l'attention des linguistes soviétiques. Signalons toutefois qu'au colloque de Moscou un certain nombre de participants ont exprimé leur désaccord avec les thèses émises par V. A. Zvegintsev et V. N. Jartseva qui, dans leur rapport, avaient limité les lois internes de développement, en ce qui concerne les changements phonétiques, aux processus qui trouvent un reflet direct dans les autres sphères de la langue et qui reçoivent un contenu grammatical.

Une idée intéressante développée dans les travaux de plusieurs linguistes soviétiques est celle des liens unissant les lois internes à la notion de «qualité», de «structure» d'une langue déterminée. Celles-ci apparaissent comme le résultat de l'action exercée par les lois internes, mais conditionnant à leur tout l'effet de ces lois et l'apparition et la manifestation éventuelles de nouvelles lois.[27]

V. N. Jartseva y insiste à plusieurs reprises, envisage ainsi, en particulier, l'exemple que nous avons déjà cité relatif à la création de verbes en anglais par passage de substantifs à la catégorie verbale, et souligne également que le caractère systématique des faits linguistiques se manifeste tant sur le plan diachronique que synchronique.[28]

Dans le même temps où ils insistent sur la spécificité du langage, où ils s'élèvent contre toute interprétation des faits linguistiques ramenant mécaniquement ces derniers à n'être que le reflet, la conséquence immédiate de faits sociaux, les

[25] V. N. Jartseva, *op. cit.* 76.
[26] V. A. Zvegintsev. *op. cit.* 195.
[27] V. A. Zvegintsev, *op. cit.* 201.
[28] V. N. Jartseva, *op. cit.* 80 et 92.

linguistes soviétiques mettent en garde contre d'autres dangers. Ces dangers sont essentiellement les suivants:

La mise en évidence de l'existence et de l'action de lois *internes* de développement des langues risque d'entraîner l'oubli des autres phénomènes sociaux avec lesquels le langage est en interrelation constante. Le danger est donc d'aboutir à une abstraction totale, à une étude du langage en soi et pour soi, en oubliant les hommes par qui et pour qui il existe. De même les linguistes soviétiques experiment leur désaccord avec toute assimilation du langage à un quelconque système de signalisation. C'est avec la plus extrême réserve, pour ne pas dire plus, qu'ils considèrent les nombreuses tentatives faites dans ce sens au cours des dernières années.

Autre danger, du même ordre, mais plus subtil: la réduction du langage à un système de concepts. Nous avons vu, déjà, comment les linguistes soviétiques distinguent, par exemple, entre la structure grammaticale, résultat d'un lent travail d'élaboration, dans le temps, et propre à telle ou telle langue donnée, et les catégories plus générales de la logique. Et c'est ainsi que V. V. Vinogradov s'élève contre la sépraration saussurienne de l'interne et de l'externe dans le développement des langues, contre les tendances structuralistes à ramener l'évolution des langues uniquement à un jeu de concepts abstraits.[29] Une même préoccupation se retrouve enfin dans l'article de V. N. Jartseva dont nous avons parlé. Elle s'exprime notamment ainsi: «Les idées des structuralistes relatives au système d'une langue constituent un modèle de fonctionnalisme pur, car selon leur théorie il apparaît que les éléments particuliers d'une langue et l'ensemble du système reposent uniquement sur les oppositions existant entre les différents éléments du système. Qui plus est, tout en affirmant, en paroles, le "caractère total" des langues, ils les représentent, en fait, comme un ensemble de petits systèmes fermés et indépendants.»[30]

Enfin les linguistes soviétiques et notamment V. V. Vinogradov dans son rapport ont exprimé leur désaccord avec certains philosophes soviétiques (Kedrov, Alexandrov) qui, tout en reconnaissant la spécificité du langage, y transposaient mécaniquement les lois ou tendances propres à tels ou tels autres phénomènes sociaux.

Aussi les linguistes soviétiques insistent-ils, sans défaillir, sur la nécessité d'étudier le langage dans sa réalité concrète, les langues dans leurs liaisons constantes avec l'histoire des peuples qui les parlent. Ainsi, tout en reconnaissant l'existence des lois internes de développement des langues, tout en appelant à entreprendre et poursuivre leur étude, ils insistent sur le fait qu'on ne doit jamais oublier qu'elles jouent dans le cadre de phénomènes plus généraux, de lois plus générales.

L'intérêt évident des recherches des linguistes soviétiques dans le domaine des lois internes de développement des langues se manifeste essentiellement sur deux plans. Dans un domaine plus général, elles apportent des précisions et des

[29] V. V. Vinogradov, *op. cit.* 40.
[30] V. N. Jartseva, *op. cit.* 97.

perspectives intéressantes relativement aux problèmes fondamentaux de la linguistique. Elles peuvent même déborder de ce cadre, comme on peut s'en rendre compte à la lecture des passages de l'exposé de V. V. Vinogradov où ce dernier examine notamment les notions de «lois» et de «développement», à la lumière des conceptions théoriques générales propres aux penseurs et chercheurs soviétiques.

D'autre part, elles fournissent des bases utiles pour l'étude concrète des différents problèmes de linguistique et de l'histoire des différentes langues. Elles peuvent ainsi faciliter les essais de périodicisation de l'histoire de différentes langues (ce qu'a tenté, par exemple, le linguiste de Sofia, Georgiev, pour le bulgare[31]). Elles permettront aussi, sans doute, d'élaborer les principes présidant aux recherches dans les domaines de la phonétique, de la grammaire et de la lexicologie historiques.[32]

Paris. Mars 1954.

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[31] Voir c.-c. et discussion dans *Voprosy jazykoznanija*, 5(1953).103ff.
[32] V. V. Vinogradov, *op. cit.* 43.

THE STRATEGY OF PHONEMICS

MORRIS HALLE*

I. INTRODUCTION. DISCRETE OR CONTINUOUS?

From the time of the invention of alphabetic writing until the end of the nineteenth century, students of language accepted without question the view that speech consisted of sequences of discrete sounds which are tokens of a small number of basic types. It was generally held that we speak in a manner quite similar to the way in which we write, and the idea that there might not be any simple, one-to-one correspondence between letters and sounds did not seem as obvious to an earlier generation as it does to us. The learned Baron van Helmont even believed that the Hebrew letters represented the position of the tongue during the production of the corresponding sounds and to illustrate this, published a set of completely imaginary articulation profiles which have a somewhat gruesome quality about them.[1]

The achievements of the natural sciences in the last century permitted as well as forced students of language to make much more detailed observations of the speaking process, with the result that grave doubts were cast on the validity of practically all the standard notions firmly held by preceding generations. Speech was shown to be not a sequence of clearly separated, discrete events, but rather a continuous flow of sound, an unbroken chain of movements.[2]

Although the view of language as a continuous phenomenon is simple and straightforward from a strictly physical standpoint, it has certain inherent difficulties which make it undesirable as a basis for descriptions, and investigators of language, phoneticians as well as physicists, have usually preferred to describe language as a sequence of discrete events.[3]

Furthermore it is not necessary that a physical phenomenon be actually discontinuous in order to break it up into a sequence of discrete events. It is possible to divide it into segments if we can show exactly how it is to be done. We shall later state some of the conditions as they apply to speech (see Chapter

* I wish to acknowledge my indebtedness to those of my colleagues and students who have served as none too passive guinea pigs on whom the ideas expressed in this essay were tested at various stages of maturation. In particular I want to thank Roman Jakobson, whose influence ought to be apparent on every page, and Noam Chomsky, whose illuminating and penetrating discussions of many problems have greatly influenced my views. This work was supported in part by the Signal Corps, the Air Materiel Command, and the Office of Naval Research.

[1] Cf. reproduction of one of his figures in H. Dudley and T. Tarnoczy, "The Speaking Machine of Wolfgang von Kempelen," *J. Acoust. Soc. Am.* **22**, 151–167 (1950).

[2] P. Menzerath and A. de Lacerda, *Koartikulation, Steuerung und Lautabgrenzung* (Berlin-Bonn, 1935).

[3] The major exception is the work of some communications engineers on long time spectra of speech; e.g. H. K. Dunn and S. D. White, "Statistical Measurements on Conversational Speech," *J. Acoust. Soc. Am.* **11**, 278–288 (1939), and L. G. Kraft, "Correlation Function Analysis," *ibid.*, **22**, 762–764 (1950).

II). At this point in the argument it is only necessary for us to establish the possibility of segmenting into discrete events the continuous acoustical phenomenon that is speech. A person taking dictation is performing just such an operation. His eardrums receive the continuous acoustical wave, his hand writes (types, if you will) sequences of discrete symbols, the letters. The acoustical wave, therefore, must contain clues which enable human beings to perform this operation. If we could state what these clues are, we could presumably build a machine to perform the same operation. In sum, both the continuous and the discrete representation of speech can—at least in principle—be achieved by a set of physical operations. We can, therefore, assert that both views are meaningful in an empirical, physical sense.[4]

It is now necessary for us to show why the discrete picture of language is preferable. Our answer is that it enables us to account for many facts which on the assumption of continuity would be extremely difficult, if not impossible, to explain. The grossest of these is the well-known fact that speech is perfectly intelligible in the presence of very great disturbances. We shall call this property of speech "resistance to noise."

In a recent paper B. Mandelbrot[5] has shown that it is impossible to account for the high resistance to noise of speech on the basis of a continuous view. If linguistic messages (utterances) be thought of as continuous, the correction of errors in the reception cannot begin until the entire message is received, which would make correction well nigh impossible, certainly infinitely more difficult than it actually is. On the other hand, if a discrete view be adopted, correction of errors can begin upon receipt of each discrete unit (quantum), since the discrete units in the language are just a small fraction of all possible things that the ear can receive.

Mandelbrot investigated in detail the consequences of the discrete character of language only on one level, that of words. The necessity for discrete units on other levels is implicit in his argument. The words themselves are thus viewed as being composed of discrete components, usually known as *morphemes*, which in turn consist of other discrete units, the *phonemes*.

The reasons for our using the phoneme as the smallest quantum of language in preference to other units that have been suggested, as, e.g., the syllable, are as follows:

(1) Since the larger units (words, morphemes, syllables) can usually be subdivided into phonemes, it follows that the number of different phonemes necessary for describing a given set of messages will be smaller than that of the larger units.

(2) The phonemes are extremely useful (if not the only possible) units when

[4] "A term (predicate) is a legitimate scientific term (has cognitive content, is empirically meaningful) if and only if a sentence applying the term to a given instance can possibly be confirmed to at least some degree." R. Carnap, "Truth and Confirmation," in H. Feigl and W. Sellars, *Readings in Philosophical Analysis* (New York, 1949) p. 123.

[5] B. Mandelbrot, "Structure formelle des langues et communication," *Word* 10, 1-27 (1954).

it is necessary to describe other facts of languages, such as rules of grammar, regularities of linguistic change, development of language in children, pathological manifestations of language, poetic devices such as rime, assonance, etc. One can easily convince oneself of this by trying to state such a simple grammatical rule as the distribution of the English past tense suffixes /-d/, /-ɪd/ or /-t/ in terms of syllables or words.

We want to insist on this point in particular. To us the major criterion for the applicability of a certain category to linguistic description is whether or not this category yields simple statements not only on the particular level for which it was introduced, but on all levels which are pertinent to descriptions of a language. It always must satisfy a multiplicity of criteria.

We have chosen to represent language as a sequence of discrete events and thereby to complicate our description on the physical level, because the physically simpler, continuous view would have made impossible other statements about language which we would like to make; e.g., to give a simple description of grammar. The nonlinguist need not follow us there, and as a matter of fact a high-fidelity recording of human speech involves neither phonemes nor words nor any other linguistic quantization.

In sum, then, the traditional view of language as a sequence of discrete events of phoneme length is empirically meaningful and has certain clear advantages not only over the purely physical view of language as consisting of utterances which themselves are continuous, but also over other proposed schemes of quantization (syllable, for instance).

II. The Phoneme

It is now necessary to sketch a procedure whereby we would be guaranteed to arrive at answers to two questions: (1) How many phonemes does our language possess? (2) What acoustical properties are used for the identification of the phonemes?

Since in our view the most important property of language is that it serves as an artifact for the transmission of information, it is evident that we must be able to record those features of the speech wave which by themselves serve to convey information. It is to be stressed that "information" is used here in its technical meaning, which involves a selection of one from an ensemble of possible messages. In other words, we must be able to detect those properties of the speech wave which enable us to tell that a particular utterance is *bill* and not *pill* or *dill* or *gill* etc.[6] We have placed ourselves hereby in the position of the receiver, and our description is to be made primarily from the receiver's point of view.[7]

In the preceding paragraph we have spoken of "an ensemble of *possible*

[6] Cf. F. de Saussure, *Cours de linguistique générale* (Paris, 1949) p. 163: "Ce qui importe dans le mot, ce n'est pas le son lui-même, mais les différences phoniques qui permettent de distinguer ce mot de tous les autres, car ce sont elles qui portent la signification."

[7] For a detailed discussion of this point see B. Mandelbrot, *op. cit.*

messages." This means that we must be prepared to deal only with those sound sequences which are possible in our language, and not with all sounds that man might emit; i.e. we need to establish the conditions sufficient for decoding the messages which we might receive, for speakers of a language are in the position of people who communicate by means of a common code. The linguist's role is in part similar to that of the cryptanalyst who must describe the code from observations made on both the messages and the behavior of the users of the code (not excluding his own).[8]

Since, if taken as physical events pure and simple, no two utterances are alike, the decision of whether two utterances are the same or different can only be made on another level, namely, by observing the behavior of the users of the code, including their verbal behavior. It has been shown by Harris[9] that this decision can be established without recourse to "meaning" or "difference of meaning." Two utterances as spoken by the same informant are recorded on tape and a test tape is prepared by re-recording the original two utterances in a random order. The test tape may thus contain fifty or sixty-re-recordings of the original utterances. The two original utterances are played to an audience of native speakers who are instructed to make a check on their ballots (or signify in some other manner) whenever they hear the first utterance during the playing of the test tape. If the utterances are "different," i.e. phonemically distinct, the audience will obtain an almost perfect score; if the utterances are homophonous the score will be in the vicinity of 50 percent. Since all linguistic descriptions are based on analyses of a finite number of utterances, it is possible, at least in principle, to subject the entire corpus to this test. This is, however, not necessary since shortcuts can be easily devised and justified. A particular advantage of this method is the neat solution which it provides for the difficulties connected with phonemically distinct utterances having the same meaning, like /ekənˈamɪks/ and /ikənˈamɪks/.

Having thus established *sameness* and *difference* among the utterances in his corpus, the linguist must next outline a procedure for the identification of the elementary discrete units that compose the utterances; i.e. of the phonemes.

In the past, the problem of identification has been approached essentially from two directions. The first approach is formulated in Steinberg and Potter's article, "Toward the Specification of Speech": "If different speakers are asked to speak one of the vowels, the utterances will, of course, be different. *The problem is to determine those physical properties that are invariant in the several utterances that enable the ear to identify them as a given vowel.*"[10] In other words,

[8] Whether the linguist can "break the code," just by investigating records without recourse to observations of the speakers' behavior, has been the subject of spirited debate. While I doubt the possibility I do not take a definite position in this matter.

[9] Z. S. Harris, *Methods of Structural Linguistics* (Chicago, 1951) pp. 32ff. It is to be noted here that in Harris' presentation recourse is had to meaning in the instructions given the original speaker, where the two utterances are identified "by translation or otherwise." On the relation between meaning and translation cf. R. Jakobson, *Sound and Meaning* (in preparation).

[10] *J. Acoust. Soc. Am.* **22**, 807 (1950).

these investigators assume that from a study of the physical characteristics of a great number of utterances of a given vowel they will be able to isolate the invariant properties which presumably serve the human being as cues for the correct identification. The attention is focused here on the properties which all repetitions of a given phoneme have in common, a sort of common denominator.

The second approach to the problem of identification was already foreshadowed in the quotation from de Saussure (footnote 6); we can add another which reads: "Or ce qui les [the phonemes] caractérise, ce n'est pas, comme on pourrait le croire, leur qualité propre et positive, mais simplement le fait qu'ils ne se confondent pas entre eux." Primary attention is here directed not to the properties which all repetitions of a given phoneme may possess in common, but rather to the properties which differentiate each repetition of a given phoneme from all other possible phonemes which might have been uttered in its stead; i.e. the focus is on the distinctive differences instead of the similarities. Focusing on the distinctive difference presupposes, of course, a knowledge of all the possible judgments which one might be expected to make; it presupposes a knowledge not only of the phoneme under investigation but also of all the phonemes in the language.

The two approaches might be illustrated by the following analogy: It is necessary to find a car in a parking lot. If it is not known what other cars are in the parking lot, then the only sufficient way to specify the wanted car is by giving its total description: make, year, model, color, and such other distinguishing characteristics as bumps, scratches and broken windshield. If on the other hand, it were known that the wanted car is the only red car in the parking lot it would be sufficient to specify this one distinctive property in order to find the wanted car. It is, of course, true that the first specification is sufficient in all cases—however, under the given conditions the second method is doubtless the more economical. In the case of language it is evident that the second method, i.e. concentration upon distinctive differences rather than upon common properties, is a more reasonable approach.

One technique for establishing the number and properties of the phonemes of a language consists in arranging the utterances in *minimally different sets*. These are sets of different utterances (preferably of short words) which differ from one another by only one phoneme but are alike in every other respect; for example, sets like:

(a) *bin, pin, kin, Lynn, tin, din, sin, thin, fin,* etc.
(b) *bin, bean, boon, bun, ban, Ben,* etc.
(c) *bin, bit, big, bill, bib, bid, bing,* etc.

It may be objected that the ability to make such an arrangement necessarily presupposes a knowledge of the solution: one cannot arrange utterances in sets in which each member differs from all others by one phoneme without first knowing the phonemes.

The objection overlooks one other way in which this particular arrangement might be arrived at, namely, by an exhaustive examination of all possible arrangements. Since the number of utterances on which our analysis is based

is finite, there is also a finite number of ways in which these can be arranged; i.e. in a finite time we could generate all possible arrangements. If we now possessed criteria for determining which arrangement actually consists of minimally different sets, we might conceivably check through all possible arrangements with the guarantee that in a finite time we should end up with the desired arrangement.

As a matter of fact we possess such criteria (see the following discussion), and it is by means of these criteria that we establish the minimally different sets. The procedure is as follows: each arrangement is provisionally assumed to contain nothing but minimally different sets; i.e. all words in it are assumed to differ from one another by one phoneme only and to be alike in every other respect. In the overwhelming majority of cases the incompatibility of this assumption with the requirements for simple solutions is immediately obvious. Thus, for example, we never consider minimally distinct, sets like *bee*, *antidisestablishmentarianism*, and *psychic*, for it is immediately apparent that no conceivable manner of segmentation of these words will yield units which even remotely resemble phonemes. (Cf. requirements of physical uniformity stated below.) In more difficult cases, as, e.g., whether the affricate [tʃ] as in *chew* should be considered as one or two phonemes (or in our terminology, whether *chew* belongs in the set *shoe*, *sue*, *too*, *do*, etc., or both in the set *true*, *tew*, and in the one-member set *chew*), both assumptions are actually tested and the interpretation yielding the simpler over-all description is chosen.[11]

The criteria which phonemes have to meet are the following:[12]

(1) They must be of relatively simple physical structure: within a phoneme segment there cannot be (a) turning on and off of the exciting source or sources; (b) a switching on and off of the nasal resonances; (c) sudden severe drops in over-all level; (d) certain changes in the formant positions; (e) sharp changes in the bandwidths of the formants (exact specifications have still to be given here).

(2) The segments considered different must be replaceable in the members of the set without destruction of intelligibility; i.e. by careful cutting and splicing it must be possible to transform minimally different utterances like *bill* and *mill* into each other, as has been done by P. Menzerath,[13] Carol Schatz[14] and others.

[11] The necessity of setting up a special set containing just the phoneme in question (since stops cannot precede fricatives in English unless the affricate be considered a cluster) would incline us towards a monophonematic interpretation. Further complications which arise in other parts of the description would cause us to prefer this interpretation even more.

[12] The first two requirements are of a physical nature and are introduced specifically to account for two facts: (a) that a fairly rough segmentation, the so-called phonetic transcription, can be made just by listening to samples of the language, and (b) the remarkable agreement of various phoneticians on how to segment a sequence. On this point see the discussion in K. L. Pike, *Phonetics* (Ann Arbor, 1943) pp. 42–55.

[13] "Neue Untersuchungen zur Lautabgrenzung und Wortsynthese mit Hilfe von Tonfilmaufnahmen," *Mélanges de linguistique et de philologie offerts à Jacq van Ginneken* (Paris, 1937), pp. 35–41.

[14] Verbal communication.

(3) There must be as many phoneme intervals in each utterance as will enable us to distinguish each utterance from each other utterance which is not a repetition of the former, and no more.[15]

(4) The identification in terms of the distinctive features (see below) must be possible and show no inconsistencies.

It is to be noted that in the above specifications there are involved both physical and distributional properties. In recent years the former have tended to be under-emphasized with the result that phonemic solutions often have had a very artificial appearance.

By application of criteria 1, 2, and 4, above, we establish where the phoneme boundaries approximately lie. However, we have not as yet identified a single phoneme.

III. Distinctive Features

It has already been stated that the method of identification to be adopted here will concentrate on the differences existing between the phonemes, and not upon the properties common to all utterances of a given phoneme. Our first step is to characterize the differences between the members of a minimally different set: we intend to state how *bin* differs from *pin*, from *tin*, from *din*, etc.

Given n entities there may be as many as $n(n-1)/2$ differences between them. This would be the case if none of them had any properties in common: e.g. if we assumed that the following eight phonemes /p/, /t/, /f/, /s/, /b/, /d/, /v/, /z/ had no properties in common, then in order to characterize each one of them by the method of differences adopted here, we would need a total of $8 \times 7/2 = 28$ statements. We would have to be prepared to answer questions of the form "Is the phoneme under consideration /p/ or /t/, /p/ or /b/, etc.?" Once we have discovered, however, that some of the phonemes possess common properties which others lack, we can pose much more "perceptive questions"; e.g. "Is the phoneme under consideration voiced? continuant?"

Since to each of the questions two answers (yes or no) are possible, we can identify by means of three questions eight different entities corresponding to the eight phonemes of our sample. Add a ninth phoneme to our example—/t͡s/, for instance—and the three questions no longer suffice. We now have a choice to complicate our description in one of two ways, either by admitting more than two answers to some or all of our questions (e.g. instead of asking "continuant? yes? or no?" we may now pose the question in the form "continuant? stop? or something in between?" thereby accepting three answers) or by increasing the number of questions (e.g. by introducing an additional "yes or no" question, like "Is it strident?"). We shall refer to the first method as an increase in the *accuracy of measurement*; we shall refer to the second method as an increase in the *dimensionality of measurement*.

In conformity with our requirement that our terms be empirically meaningful,

[15] This is a slightly paraphrased version of a statement by Z. S. Harris, *op. cit.* p. 43. On homophones see *ibid.*, pp. 32-ff.

the "questions" in the preceding paragraph are of a kind to which answers can be provided by physical measurement; i.e. they are questions regarding the presence of certain definite physical properties. Thus if we ask whether a certain phoneme is voiced we have in mind definite acoustical measurement procedures to determine it.[16]

It is to be noted that the number of questions is considerably smaller than the number of phonemes. The minimum number of "yes or no" questions necessary to identify n phonemes is $\log_2 n$. In the language of communications engineering such a description would be said to possess minimal redundancy.[17] In a natural language we should not expect minimal redundancy, for redundancy is one of the factors that makes language resistant to noise. On the other hand, we would expect natural languages not to have excessive redundancy, for a very redundant language is an inefficient language.

The physical properties whose discovery is the purpose of the "questions" of the preceding paragraphs have received a detailed discussion in another place.[18] They are referred to there by the term *distinctive features*, which will also be used in the remaining part of this essay.

It may justifiably be asked: "How does the linguist know what 'questions' to pose?" The answer is much like the one we gave when we discussed the establishment of phoneme boundaries. As yet, we have no procedure for arriving at the correct set of "questions"; we can only guess. We do, however, possess a method for checking whether or not any proposed set fits our requirements for a simple description of language on all levels.

Many distinctive features, though under different names, have long been used in phonetics. They are implicit in the work of the Hindu grammarians as well as in the works of Western phoneticians. In spite of their apparently independent development these two major traditions show certain striking similarities. Both traditions agree in assigning great importance to the "point" of articulation, the place in the vocal tract where the stricture is narrowest. They also agree to some extent in their treatment of the "manner" of articulation and of nasality. There is, however, one striking difference between the two traditions. The Hindus used the "point" of articulation for the classification of both the vowels and the consonants, while in the West the vowels were classified according to the so-called "vowel triangle" (a two-dimensional classification).

Thus the picture which the classification of the sounds presented was a mixed one: most features were two-valued (for example, sounds were said to be either nasal or non-nasal; voiced or unvoiced; aspirated or unaspirated, etc.); a few others were multivalued. Further complications were introduced in the course of the nineteenth and twentieth centuries when it was discovered that sounds

[16] For details of the acoustical measurement procedures see M. Halle and L. G. Jones, *The Russian Phonemes* (in preparation).

[17] On the concept of redundancy see C. Shannon and W. Weaver, *The Mathematical Theory of Communication* (Urbana, 1949) and R. Jakobson, C. G. M. Fant, and M. Halle, *Preliminaries to Speech Analysis, Technical Report No. 13*, Acoustics Laboratory, M.I.T., May 1952, pp. 4–8.

[18] R. Jakobson, C. G. M. Fant, and M. Halle, *loc. cit.*

which had been thought to differ only in their manner of articulation differed also in their place of articulation. There were even suggestions in the literature to make the place of articulation the primary variable and to consider the other features as redundant. This led, however, to complications, once it was actually put into practice. For example, in French it may be said that the difference between /g/ and /ɲ/ lies in their different points of articulation: /g/ is velar and /ɲ/ prepalatal. It was thought that by this increase in the number of points of articulation, the introduction of an additional dimension, i.e. nasality, would be avoided. French also possesses the phonemes /b/ and /m/, which have identical points of articulation. The dimension of nasality, therefore, must be introduced anyway to distinguish between /b/ and /m/ and might as well be used in the case of /g/ and /ɲ/, for it is patently uneconomical to describe the labial consonants differently from the velars, especially if it is possible to avoid this.

These difficulties were resolved by Roman Jakobson.[19] In making descriptions one usually has the choice between using few dimensions with many significant values and using many dimensions with few significant values. In other words one can trade accuracy of measurement for dimensionality. Reduced to its simplest terms, Jakobson's fundamental argument was that the most satisfactory description of a language would be obtained by using as many dimensions (features) as necessary, but decreasing the accuracy of measurement, i.e. the number of significant decisions which have to be made with regard to each dimension. The dichotomous scale, which underlies the distinctive features, has minimal accuracy of measurement: it is in this sense that it is the simplest possible.[20]

Jakobson suggested that the consonants be subdivided into "strident" vs. "mellow." He thus obtained a fourfold division of consonants where formerly there was only a twofold one: the stops and the continuants were each subdivided into strident and mellow. The increase in dimensionality brought with it a corresponding decrease in the number of "significant" points of articulation. (As a matter of fact it was shown that four such points sufficed for all languages.)

Then, as if taking his cue from the old Hindu grammarians but turning them upside down, Jakobson ordered both vowels and consonants according to a single principle, which, in conformity with his fundamental demand for a substitution of dimensionality for accuracy, he found in the two-dimensional vowel triangle rather than in the one-dimensional, multivalued "points of articulation" parameter.[21]

Finally Jakobson demanded an acoustical classification of the sounds of speech. *Preliminaries* is a step in the implementation of this program, for there

[19] R. Jakobson, "Observations sur le classement phonologique des consonnes," *Proceedings of the Third International Congress of Phonetic Sciences* (Ghent, 1939) pp. 34–41, and *Kindersprache, Aphasie und allgemeine Lautgesetze, Språksvetenskapliga sällskapets i Uppsala förhandligar* (1940–1942).

[20] I. Pollack's recent work in J. Acoust. Soc. Am. **25** (1953) and **26** (1954) (see footnote 24 below) provides interesting evidence relevant to this problem.

[21] For details see *Preliminaries* and R. Jakobson's forthcoming *Sound and Meaning*.

the attempt is made to state all the dimensions of phonetic descriptions in terms of acoustical as well as articulatory criteria.

Traditional phonetics was primarily an articulatory phonetics. From time to time attempts were made to translate the articulatory terms into acoustical. Since results were not always convincing, doubts were voiced as to whether this could be done at all.[22] There are two arguments in favor of the possibility of translating into acoustical terms uniformities which have been observed most clearly and first on the articulatory level. The first argument is quite simple: given a certain geometrical configuration and excitation of a resonator, its acoustical output is entirely predictable. Hence, all other things being equal, any uniformity on the articulatory side *must* have a statable acoustical counterpart. The second argument appeals to the fact that the phoneticians who picked a particular articulatory uniformity as a distinctive feature over a whole series of others which they might have chosen were guided by the observation that these functioned as perceptually distinctive marks, and hence must also have existence on the acoustical level.

The distinctive features in the formulation which is given in *Preliminaries* are, with one exception, binary in structure. This is to be understood as an empirical proposition: If the differentiating phonemes in a minimally different set were to be measured for a certain distinctive feature (e.g. degree of voicing), the results would cluster about two values: one for the voiced and the other for the unvoiced consonants. E. Zwirner's experiments with German vowels[23] do not constitute a counter-example that proves the incorrectness of the hypothesis of binarity, because Zwirner did not compare vowels in identical context only (he plotted all vowels in his sample on a single graph) and the above proposition applies only to minimally different sets, i.e. to contexts identical by definition.

The success of the distinctive features as a framework for linguistic descriptions in terms of which a host of difficult linguistic problems can be readily explained is the major reason for their adoption. Recent work in psychoacoustics has provided further support for the distinctive feature model. Studies by Pollack and others on the transmission of information by multidimensional auditory stimuli show that best results "are obtained when each dimension [is] crudely subdivided into two alternative states. Finer subdivision of each dimension does not produce a proportional gain in information transmission with the display."[24] It is only reasonable to assume that natural languages are constructed in a way fairly closely approximating optimal conditions for auditory transmission of information among human beings.

[22] Cf. E. Fischer-Jørgensen, "The Phonetic Basis for Identification of Phonemic Elements," *J. Acoust. Soc. Am.* **24**, 14–15 (1952).

[23] E. Zwirner, "Phonologische und phonometrische Probleme der Quantität," *Proceedings of the Third International Congress of Phonetic Sciences* (Ghent, 1939) pp. 57–66.

[24] I. Pollack and H. Ficks, "The Information of Elementary Multidimensional Auditory Displays," *Program of Meeting of the Acoustical Society of America, B, 2,* May 7–9, 1953, Philadelphia. This paper has now been published in full in *J. Acoust. Soc. Am.* **26**, 155–158 (1954).

To justify still further the use of the distinctive features as descriptive parameters we have given in *Preliminaries* under the sub-heading "Occurrence" examples from the most diverse languages in which the distinctive features provide a convenient framework for the expression of observations which otherwise would require much more complicated statements.

Finally comparisons have been made between the economy of a code utilizing the distinctive features and that of a theoretical code having minimal redundancy. The distinctive-feature code is, as expected, somewhat lower in economy than the optimal code, but not very far below it.[25]

IV. Procedure of Analysis

The procedure of analysis by means of the distinctive features is as follows: The segments which were established as signalizing phonemic differences are subjected to an analysis in terms of the entire list of distinctive features. We obtain answers to questions such as: "Is the segment under consideration voiced? Is it a continuant? Is it fortis (aspirated)?. . . ." through the entire list of features. Each segment in each of the words in our catalogue is so characterized. Segments which have the same answers are said to be the same phoneme[26] and are symbolized by the same letter.

In the course of this analysis it will turn out that certain questions are not necessary for identification: i.e. in the language under consideration there will be no pair of words which are distinguished by the differences which the given question has in view. Thus, for example, we shall find that in the set *bin*, *pin*, *din*, *tin*, etc. all voiceless stops are fortis (aspirated), while in the set *spin*, *skin*, etc. none of the voiceless stops is aspirated. Since this difference is associated with different contexts it is not a primary but a redundant difference and can, therefore, be disregarded (at least as far as stops are concerned) for the present purpose, which is to establish the minimal conditions for identification.

On the other hand, if in a given minimally different set a certain distinction is not represented, it cannot be disregarded if it functions distinctively in another context: e.g. in English there is no *stin* opposed to *spin* although the difference between /p/ and /t/ functions in many other contexts, and hence a statement about the quality of gravity (distinctive feature characterizing the difference between /p/ and /t/) must be made.[27]

The method of minimally different sets avoids the difficulties connected with allophones and eliminates the need for reference to "phonetic identity" as was already pointed out in 1935 by Twadell.[28] Since the members of minimally

[25] Cf. E. C. Cherry, M. Halle, and R. Jakobson, "Toward the Logical Description of Languages in their Phonemic Aspect," *Language* **29**, 41 (1953).

[26] This constitutes a definition of "phonetic similarity."

[27] In cases where a feature is neutralized in a certain context, as e.g. in English in the above example, the question arises: To which phoneme, /p/ or /t/, is the above consonant to be assigned? I believe that this is to be decided by physical measurement.

[28] W. F. Twadell, *On Defining the Phoneme, Language Monograph No. 16* (1935). See also R. Jakobson, "On the Identification of Phonemic Entities," *Travaux du Cercle Linguistique de Copenhague V*, 205–213 (1949).

TABLE I

Matrix showing the phonemes and the distinctive features of standard literary German

	m	p	b	f	v	f̂	n	t	d	s	z	ŝ	k	g	x	ʃ	r	l	u	o	a	ü	ö	i	e	æ	h
Vocalic vs. non-vocalic..	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+	+	−
Consonantal vs. non-consonantal.....	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	−
Compact vs. diffuse.....	0	−	−	−	−	−	0	−	−	−	−	−	+	+	+	+	0	0	−	±	+	−	±	−	±	+	0
Grave vs. acute.....	+	+	+	+	+	+	−	−	−	−	−	−	+	+	+	−	0	0	+	+	+	−	−	−	−	−	0
Flat vs. plain.....	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	+	+	0	+	+	−	−	0	0
Nasal vs. non-nasal.....	+	−	−	−	−	−	+	−	−	−	−	−	−	−	−	−	0	0	0	0	0	0	0	0	0	0	0
Continuant vs. interrupted.....	0	−	−	+	+	−	0	−	−	+	+	−	−	−	+	0	−	+	0	0	0	0	0	0	0	0	0
Strident vs. mellow.....	0	−	0	0	0	+	0	−	0	0	0	+	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Tense vs. lax.....	0	+	−	+	−	0	0	+	−	+	−	0	+	−	0	0	0	0	0	0	0	0	0	0	0	0	0

The symbol ± indicates an intermediate degree of compactness, for the feature of compactness is ternary for vowels.
The vowels are further differentiated into long vs. short. Among short vowels the distinction between /e/ and /æ/ is non-phonemic.

different sets are by definition identical contexts, we are always comparing things which are otherwise the same, and the question of identifying phonemes purely by complementary distribution is entirely eliminated. The much discussed problem of whether we should consider the English [h] and [ŋ] as one or two phonemes does not arise at all, because the answers from analyzing *hill* in a minimally different set like *hill, pill, till, bill,* etc. differ completely from those obtained by analyzing *sing,* in a set like *sing, sin, sick, sit,* etc.

V. Identification of Phonemes

The analysis just described permits us to establish the inventory of the phonemes of the language. We can write our results in the form of a matrix in which each phoneme is given with the distinctive features which are necessary for its identification.

The identification of phonemes can then be thought to proceed as follows:

(1) The speech is segmented according to principles outlined above.

(2) Each segment is analysed in terms of distinctive features. In order to establish the correct answer it may often be necessary to refer to adjacent segments.

(3) Each segment is identified by reference to the matrix (see table) with the following instructions: If the analyzer output for any given feature is positive, disregard all phonemes which in the matrix are marked negative for that feature. If the analyzer output is negative for any given feature, disregard all phonemes which are marked positive for that feature. Do nothing about phonemes for which the particular feature is not distinctive (i.e. which have noughts in the table).[29] At the end of such an analysis there will be only one phoneme which has not been excluded from consideration—it is the phoneme under analysis.

VI. Conclusion: Fundamental Properties of Natural Languages

The model of language which has been presented here has the following properties:

(1) Language consists of discrete units of short duration which meet certain physical requirements (the phonemes).

(2) In their function as signaling devices the phonemes can be viewed as simultaneous implementations of a number of attributes—the distinctive features.

(3) The distinctive features are, with a single exception, binary.

(4) No language utilizes all the distinctive features.

(5) No language has as many phonemes as there are possible combinations of the utilized distinctive features.

Massachusetts Institute of Technology

[29] This provision does two things: it establishes the status of the noughts in our matrix tables and makes allowance for what is known as "free variation," where a feature, being nondistinctive, may or may not be present in various utterances of a given phoneme, e.g. nasality in vowels in Midwestern dialects of American English.

TWO MODELS OF GRAMMATICAL DESCRIPTION

CHARLES F. HOCKETT

I. INTRODUCTION

1.1. By a 'model of grammatical description' is meant a frame of reference within which an analyst approaches the grammatical phase of a language and states the results of his investigations. In one sense, there are as many models as there are different descriptions ('grammars' in the sense of monographs). But in another, and very important, sense, most grammatical descriptions seem to cluster about a relatively small number of relatively distinct models; it is with these archetypical frames of reference that we are concerned here.

The bulk of the present paper was written between 1949 and 1951; at that time, the writer was under the erroneous impression that there were principally just two archetypes to be dealt with. If we confine our attention to monographs produced in the United States in the past half-century, this impression is not grossly inaccurate. One of the 'two models' which is well represented within those spatial and temporal limits is what we shall call ITEM AND PROCESS, or IP for short; the other we shall call ITEM AND ARRANGEMENT, or IA. In most of what follows we shall have occasion to mention only these two.

But that limitation constitutes a defect in the paper—a defect which was recognized by the writer in 1951, and because of which the paper was long held unpublished. Quite apart from minor variants of IP or IA, or models that might be invented tomorrow, there is one model which is clearly distinct from either IA or IP, and which is older and more respectable than either. This is the WORD AND PARADIGM (WP) model, the traditional framework for the discussion of Latin, Greek, Sanskrit, and a good many more modern familiar languages. It will not do to shrug this frame of reference off with the comment that it is obviously insufficiently general, incapable of organizing efficiently the facts of a language like Chinese. As yet we have NO completely adequate model: WP deserves the same consideration here given to IP and IA. The writer offers his apologies for not having worked such consideration of WP into the present paper. However, lack of time prevented this, and the discussion as it stands may nevertheless be of some value.

1.2. As between IP and IA, the former is older. The key term in IP is 'process', as is evident from the following characterization by Harris (1944, p. 199):[1] "The difference between two partially similar forms is frequently described . . . as a process which yields one form out of the other. Thus, when bases or themes have several vocalic forms, the various forms are said to be the result of vowel-change processes operating upon the base or theme. The difference between a base and a base-plus-suffix is described as a result of the process of suffixation. This is a traditional manner of speaking, especially in American Indian grammar.

[1] Reference is made by author and year of publication to the bibliography at the end of the article.

It has, of course, nothing to do with historical change or process through time: it is merely process through the configuration, moving from one to another or larger part of the pattern."

Rigorous work with historical linguistics, as everyone knows, preceded almost all rigorous descriptive work; the carry-over of 'process' terminology from historical discussion is natural enough. In this country Boas (1911, pp. 27f.) established IP, and Sapir (1921, esp. ch. 4) elaborated it; the descriptive chapters of Sapir's *Language* are cast entirely in this mold. Grammars written largely under Sapir's aegis, such as Newman's Yokuts (1944; the specific aim of Harris's remarks quoted above), still stand as examples of IP in action.

1.3. As a further example of IP, consider the following paragraph from Haas's shorter treatment of Tunica (1946):

> The Tunica language is mildly synthetic in structure. In its technique of synthesis it is for the most part agglutinative, but it also employs a limited amount of fusion. The morphological processes used are juxtaposition, affixation (prefixation, infixation, and suffixation), reduplication, and suppletion. Of these, prefixation and suffixation, particularly the latter, are exploited to a greater extent than are the other processes.

There is no question about the meaningfulness of this characterization. Whether it is particularly relevant, either to a description of Tunica or to a proper placing of Tunica in the gamut of linguistic types, and whether, relevant or not, it is effectively phrased, are other matters.

1.4. The younger model, IA, has been formulated at least in part because of a feeling of dissatisfaction with the 'moving-part' or 'historical' analogy implicit in IP. At the very least, those analogies seem to imply the necessity of making certain decisions in a possibly arbitrary way. Critics of IP would prefer to circumvent such decisions altogether.

For example (cf. Hockett 1947a, pp. 282–3), if it be said that the English past-tense form *baked* is 'formed' from *bake* by a 'process' of 'suffixation', then no matter what disclaimer of historicity is made, it is impossible not to conclude that some kind of priority is being assigned to *bake*, as against either *baked* or the suffix. And if this priority is not historical, what is it?[2] Supporters of IP have not answered that question satisfactorily.

Another objection which might be raised to IP as exemplified in the above quotation from Haas is that the wording tends towards personalization and teleology. The Tunica language 'uses' or 'employs' various techniques; it 'exploits' some of those more than others. The use of 'the Tunica language' as a subject with such verbs is personalization; the use of such verbs seems to imply teleology—the language has a purpose to accomplish, and it makes use of such-and-such means to the end. If any entity 'uses' the techniques, it is the speakers, not the language. This objection is pointless unless it can be shown that such a way of speaking leads its users into errors of fact. So long as it is

[2] The possible criteria are examined by Pittman, 1948. But Pittman's discussion is cast in IA, and affords no obvious support for the IP approach.

merely a 'way of speaking', easier than some other way because English is an Indo-European language, we cannot object.

1.5. The essence of IA is to talk simply of things and the arrangements in which those things occur (Harris 1944, section 5, esp. end of page 203; Bloch 1947, introductory remarks; Harris 1945b; Harris 1942). One assumes that any utterance in a given language consists wholly of a certain number of minimum grammatically relevant elements, called morphemes, in a certain arrangement relative to each other. The structure of the utterance is specified by stating the morphemes and the arrangement. The pattern of the language is described if we list the morphemes and the arrangements in which they occur relative to each other in utterances—appending statements to cover the phonemic shapes which appear in any occurrent combination.[3]

The matter is not quite so simple as that. Some of the complexities will concern us later. Here it must be noted that there is some indeterminacy even in the apparently trivial matter of deciding what to subsume as 'item' and what to call 'arrangement'. In English, intonation phonemes can be taken as comprising parts of morphemes just as do vowels and consonants. There is then a set of morphemes composed entirely of intonation phonemes, and such intonation morphemes occur, not before or after other kinds of morphemes, but simultaneously with them. In *Come here!*, the segmental form and the /(2)31/ intonation morpheme occur at the same time. But this is not the only way to handle the matter. An alternative is to consider pitch as a second dimension in which (segmental) forms can be arranged, there being four possible positions along this dimension, in contrast to the infinite number along the time axis. For purposes of demonstration, we could then write

(4)
(3) *h*
(2) *come* *e*
(1) *re*

—where the numbers specify the pitch-levels. There would then be no intonation morphemes, but rather a two-dimensional space in which other types of morphemes are arranged relative to each other.[4] The four stress levels of English

[3] Floyd Lounsbury has suggested (private conversation) that we can profitably speak of the 'structure' of a word or utterance but of the 'pattern' of a language. This suggestion is followed here. (The contrast, of course, parallels one of those designated by the terms 'parole' and 'langue'.)

[4] The problem of dimensionality is confusing. Recognizing intonation morphemes, we have to accept simultaneity as one arrangement of morphemes relative to each other. This, in turn, suggests that there is a second dimension of arrangement in addition to time; but there are no contrasts between different 'arrangements' in this second dimension—the term 'superfix' is no more apt than 'subfix', and (the bad Latin) 'simulfix' might be even better. A dimension in which arrangements are not in contrast is rather thin. Lounsbury has suggested (private conversation) that it is better to speak of just one dimension, with simultaneity as one possible arrangement within it. If we treat intonations (or stresses) as features of arrangement instead of as morphemes, then clearly we have more than the one dimension time; but in this case, although two items can occur at dif-

could be handled in the same way, giving three dimensions of arrangement. Bloomfield (1933, p. 163) chooses this alternative when he speaks of taxemes of 'modulation'.

IA is present implicitly, though not with complete clarity, in Bloomfield's chapters on grammar; where traces of IP survive, he is sometimes a bit apologetic (1933, p. 213 top; cf. Wells, 1949, p. 102). It is certainly Bloomfield's systematization which has served as the main point of departure for the train of investigators who have been trying to develop IA—Harris and his students, Bloch, Wells, lately Nida, and others (see bibliography passim under these names, Nida only in 1948, 1949. Nida 1946 is cast essentially in IP). There are few full grammatical descriptions which illustrate IA in its purest form; Bloch's discussions of Japanese are probably the closest (1946; even more—work done under Bloch's direction —Gardner 1950, Yokoyama 1950).

1.6. There is partial translatability between IP and IA, but the results of translation are apt to seem somewhat strange. By way of demonstration, here is the passage from Haas's Tunica, recast in IA:

> The average number of morphemes per word in Tunica stands about midway between the highest and lowest averages known from various languages. The morphemes within a single word are for the most part relatively invariant in shape, but there are some cases of more alternations. Tunica morphemes fall into several position-classes: stems, prefixes, infixes, and suffixes; at least one affix [we are not told which kind or how many] is morphophonemically a chameleon. A single word may contain more than one stem. Suffixes are more numerous, both as a class and as regards the average number per word, than prefixes, and the latter in turn more numerous than infixes. In a few cases, morphophonemic alternations are between totally dissimilar forms.

The original, in all seeming, imparts relevant general information about Tunica. The translation given above has little or no such impact—at least on this reader; it has the appearance, rather, of an assembly of prosaic facts, which have no particular reason for being given in the sequence in which they are here found, or, indeed, even for being given all at one place.

If this be true, then there must be more difference between the IP and the IA models than has yet been touched on; there must be major differences as to what features of a language are regarded as worthy of prominent mention and what features are prosaic—even if necessarily included somewhere in a reasonably complete description.

This last comment is offered not as a forerunner of greater elaboration to follow, but in lieu thereof. There are differences between IP and IA which will have to be ignored within the fairly formal framework that will shortly be built, and it would be wrong to ignore them altogether.

1.7. It seems to me that the current general preference for IA rather than IP —and such a prejudice is certainly observable—stems at least in part not

ferent times on the same intonation level, two items cannot occur at the same time on different intonation levels. There is still something rather queer about the added dimensions.

from any great excess of merit of IA over IP, but rather from the following: (1) We like, nowadays, to be as formal as possible. (2) IA has been formalized, and IP has not. It is unfair to compare a formalized IA with an informal IP and conclude that the former is better just because it is formalized. If it could be shown that IA is capable of formalization but that IP is not, that would be another matter. But in what follows, I hope to demonstrate that no such claim can be made.

To do this, it is of course necessary to devise a formalized version of IP. This will be almost our last step. It will be preceded by: a general outline of IA, in broad enough terms to cover most, if not all, of its current varieties; a survey of some of the problems implicit in IA (in two parts: tactical and morphophonemic); an excursion into certain elementary notions of mathematics, seeking analogs for IA and other possibly useful analogs. Our formal version of IA will be derived from such an analog. We will then be in the position to assess the relative merits and defects of IA and IP.

II. Item and Arrangement

2.0. The following outline of IA might be called 'semiformal'—not so formal as to qualify as 'postulates', but succinct rather than discursive. The model is familiar, so all but the most essential examples are eschewed.

2.1. A linguistic form is either SIMPLE or COMPOSITE.[5]

2.2. A simple form is a MORPHEME.

2.3. A composite form consists of two or more IMMEDIATE CONSTITUENTS standing in a CONSTRUCTION and forming a CONSTITUTE. Constituents and construction recur in other composite forms (save for an occasional unique constituent). Each IC (=immediate constituent) occupies a certain POSITION in the construction; each is the PARTNER of the other(s).[6]

2.4. Occasionally it is convenient to regard a morpheme not as participating in any construction, but rather as a MARKER of the construction in which nearby forms stand.

Any such concession necessitates a statement of the conditions under which the interpretation is to be accepted. Three such statements of conditions occur to me, the first being the trivial (but not unimportant) one of excluding the interpretation altogether.

The second is to allow the interpretation only where the marker indicates the boundary between partners in a construction, and nothing more. By this, the Peiping Chinese junctural morphemes /;/ and /,/ are allowable as markers, since in such an utterance as /ue^3 lai^2, ni^3 zieu^4 cy^4./ 'When I come, you go right away', the /,/ tells us that the segmental sequence before it and that after it are partners in a construction—that is, that this division of the whole sequence,

[5] The term 'linguistic form' is here to be regarded as being defined by the other terms at least as much as they are by it. The outline assumes, without explicit statement, a certain amount of our linguistic common sense.

[6] The only new term is 'partner', which I venture to suggest to fill a hole in our terminology in which I have stumbled more than once.

rather than any other, is the correct one—but it does not tell us what construction is involved (Hockett 1950, section 2).

This limitation, however, would not allow us to interpret *men and women* as a bipartite construction with marker *and*, since *and* quite obviously gives us more information than the maximum amount specified. A set of conditions which would allow the interpretation in this case would have to be along the following lines: there is a form ABC consisting of three smaller forms, where (1) one of the smaller forms, say B (it would not have to be the one in the middle) is a single morpheme; (2) the remaining two, A and C, are structurally similar to each other (in some appropriate sense) but not to B; (3) there is no evidence forcing either an interpretation as A|BC or as AB|C.[7]

Even more liberal sets of conditions might be formulated. Our reason for including the present discussion will appear in the sequel.

2.5. The TACTICAL PATTERN of a language is completely covered by a set of statements of the following form (or by any set of statements which can be transformed mechanically into this form):[8]

(1) A list of the constructions.
(2) Under each construction as heading,
  (2.1) Enumeration of the positions in that construction.
  (2.2) Specification of any marker for that construction.
  (2.3) For each position,
    (2.31) A list of the morphemes which occur there, and
    (2.32) A list of the constructions, composite forms belonging to which occur there.

2.6. A morpheme may appear in more than a single phonemic shape. A single shape of a morpheme is a MORPH; the various morphs which are the shapes or REPRESENTATIONS of one and the same morpheme are its ALLOMORPHS.

2.7. The alternations in shape of a morpheme are predictable in terms of the environments in which it occurs (plus, of course, the morphophonemic statements which one makes).

This necessitates a definition of 'environment': the environment of a morpheme-occurrence is the setting of that occurrence, insofar as that setting can be described in purely structural (i.e., nonsemantic) terms. Narrower definitions are possible; ours is intentionally wide.

2.8. All the phonemic material in an utterance is accountable for in terms of

[7] This incorporates Wells's specification of the conditions under which one resorts to a tripartite instead of a bipartite construction (Wells 1947, pp. 103f.).

[8] Many different arrangements of descriptive statements are convertible into this form. In particular there is the contrast between beginning with the most inclusive constructions and working down the scale (large-to-small), which is perhaps the traditional way at least for syntax (exemplified, e.g., by the arrangement of the grammatical chapters in Bloomfield 1933), and beginning with the least inclusive constructions and working up the scale (small-to-large), the procedure proposed by Harris, 1946. These two ways have in common—despite Harris's disclaimer in section 3.7 and fn. 8—the recognition of IC or hierarchical structure, though Harris's procedure discovers that structure step by step, whereas the alternative seems to imply discovering it in advance.

the morphemes which compose the utterance and the arrangement in which they occur relative to each other (plus, once again, one's morphophonemic statements).

2.9. Subject to the conditions just stated (2.6–8), it is possible to allow such devices as the following where they prove convenient:

(1) We may recognize phonemic zero as a possible morph shape. This requires carefully formulated supplementary criteria. At least two apparently consistent limitations have been proposed:

(1.1) A morpheme does not appear everywhere as zero (Bloch, 1947, section 2.3, (3)).

(1.2) A morpheme may appear everywhere as zero, providing it is a member of a tactically relevant class (under some suitable definition), other members of which are not (or are not always) represented by zero (Nida 1948, 1949; the criterion is there by implication, though not overtly stated).

(2) We may recognize a morph which belongs to no morpheme (Hockett 1947b, p. 333). Or, instead of this, we may simply classify some of the phonemic material in some utterances as NONMORPHEMIC (Hockett 1950, fn. 8).

(3) We may recognize a morph which belongs simultaneously to two (or more) morphemes in a fixed arrangement (whether partners in a construction or not). If the morphemes involved appear, in other environments, in separable shapes, then the morph in question is a PORTMANTEAU representation of the morpheme-group (Hockett 1947b, p. 333). If the morphemes involved appear only in such portmanteau representations, the usual terminology is a little different: the representations themselves are called 'morphemes', and the separate entities represented are called 'morphemic components' (Harris 1948, more extensively in 1951). This more usual, but deviant, terminology will not be used in the present discussion.

(4) We may allow a particular phoneme or sequence of phonemes to belong at one and the same time both to a morph which includes some preceding phonemes (and represents one morpheme) and to a morph which includes some following phonemes (and represents another morpheme)—LINKED MORPHS, the shared phoneme(s) being a LINK (Wells 1949, fn. 29, with credit to Lounsbury). Trivially different is the case in which one of the morphs, instead of both, contains phonemic material which is not part of the other.

2.10. The statement of shapes, alternations, and conditions of alternation describes the MORPHOPHONEMIC pattern of the language.

2.11. Morphophonemic and tactical pattern taken together constitute GRAMMATICAL PATTERN. This, paired with PHONOLOGICAL PATTERN, completes the synchronic pattern of a language. The cleavage between phonology and grammar is thought by some (including myself) to be more fundamental than that, within grammar, between morphophonemics and tactics, even though, for some purposes, other stratifications are possible. For example, it is sometimes convenient to class morphophonemic and phonological facts together, say as 'mechanics', in collective contrast to tactics (Hockett 1948, p. 185). Or, if the

distinction between automatic and non-automatic morphophonemics is made (we have not taken the trouble to work it into the above outline), it also makes a good deal of sense to class automatic morphophonemic facts with phonology, non-automatic with tactics; I have no labels to propose for the resulting two divisions (this is the stratification which seems to be implied by certain of Sapir's discussions: 1933, 1930). In any case, there are indeed problems about the exact location of the lines of demarcation between levels, but we need not concern ourselves with them here because the formal version of IP to be presented later will solve them neither more nor less than does IA.

III. Tactical Difficulties With IA

3.0. A grammatical description built according to the plan outlined in II sets forth principles by which one can generate any number of utterances in the language; in this sense, it is operationally comparable to the structure of that portion of a human being which enables him to produce utterances in a language; i.e., to speak. It is also comparable to a cookbook. From the lists in the tactical description, choose any set of compatible ingredients. Put them together, two by two or few by few (ICs), until all have been tentatively assembled; then treat in accordance with the morphophonemic statements (in cooking, 'cream', 'blend', 'broil', etc.); remove traces of these operations (clean and put away the cooking utensils); and one has an utterance in the language. In cooking, a difference in the sequence in which ingredients are put together can make a difference in the end-product; if this were not so, manufacturers of processed foods would not announce, as they sometimes do on packages, both ingredients and proportions. But there can also be differences of sequence in cooking operations which make no discernible difference in the end-product. For the moment this second fact is of primary relevance for the analogy. There is, in linguistics, no guarantee that different sequences of operations, performed on the same ultimate constituents, may not produce the same, or what is ostensibly the same, end-product.

In fact, we can specify the conditions under which this will be the case. The conditions involve the constructions recognized in the tactical description and the forms listed under the various positions for the various constructions. Let there be four constructions, C_1, C_2, C_3, and C_4 (some of which may be the same). Let there be three forms F_1, F_2, and F_3. Suppose that F_1 and F_2 occur in that order in C_1; F_2 and F_3 in that order in C_2. Suppose that forms conforming to C_1 (hence F_1F_2) occur in the first position of C_3, and that F_3 occurs in the second position of C_3. Finally, suppose that forms conforming to C_2 (hence F_2F_3) occur in the second position of C_4, and that F_1 occurs in the first position of C_4. Under just these conditions, the composite form $F_1F_2F_3$ will be a case in point.

We may illustrate with English. C_1 and C_4 are the construction of adjective-attribute and nominal-head. F_1 is *old*, F_2 is *men*. C_2 and C_3 are the marked construction *X and Y*; F_3 is *women*. Consequently we may build the composite form *old men and women* by either of two procedural sequences: putting *old* and

men together by C_1, and then joining the result to *women* by C_2; or putting *men* and *women* together by C_2, and then putting *old* together with the result of C_2 in accordance with C_1.[9]

The converse of the cookbook nature of a grammatical description is that, when presented with an utterance in a language the pattern of which has been determined, the analyst should be able to state its structure. Now it is clear that in any such case as that of *old men and women*, the analyst will be able to determine the ultimate constituents and the (linear) order of those constituents, but he will not necessarily be able to tell the order of association, the IC-structure or HIERARCHICAL STRUCTURE, of the utterance. The fact that his description supplies more than a single procedural sequence which will build the composite form implies that when presented with the whole form the analyst cannot determine which procedural sequence was involved in its production.

We therefore have to make a decision. It was mentioned above that a grammatical description is an operational parallel to part of a speaker's internal apparatus. If we believe that this parallel extends to the matter of order of association, so that ambiguities in the description are matched (at least in some cases) by distinct internal chains of activity which produce identical linear sequences of morphemes, then we can regard the matter of hierarchical structure as an integral part of the structure of utterances even in ambiguous cases. But if we do not so believe, or if we feel that such a conclusion ought not to influence our development of analytical techniques, then we are forced to conclude that hierarchical structure is at most a convenient shorthand for description—not anything essential about the grammar of a language, but simple a way to make our description of a whole language less cumbersome—an intrusive artifact, like the dye on a bacteriologist's slide. For, obviously, it would be possible (save for lack of time) to specify all arrangements of ultimate constituents in utterances without resort to any intermediate groupings.

The IA picture is therefore potentially somewhat more complex than our original emphasis on the two notions ('item' and 'arrangement') would imply. There are more potentially independent factors than those two. Specifically, there are (1) forms, (2) (linear) order, (3) constructions, and (4) IC or hierarchical structure. We must investigate the status of these factors relative to each other. Are they all 'primitives' in the system, or are some derivable from others?

The independence of forms and order is clear almost without demonstration. Such a trivial example as *John hit Bill* versus *John hit Dick* demonstrates the independence of form from order; the former versus *Bill hit John* demonstrates the independence of order from form, since these two could be said to differ

[9] Wells 1947, p. 93. Since normal English orthography leaves out certain grammatically relevant features of utterances (e.g., intonations), any single English notation, such as '*old men and women*', subsumes ambiguously a family of linguistic forms. Some members of this family are marked clearly as having one IC structure; some are marked equally clearly as having the other. But, at least for my own speech, there are also members of the family that are not marked in either way. In the context, it is of course one of the latter with which we are dealing; here, and in similar situations later, the reader must assume that this is the case.

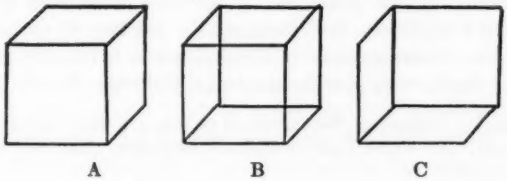
only as to order of ultimate constituents. But the status of constructions and of hierarchical structure needs further discussion.

3.1. Beginning with hierarchical structure, let us consider again Wells's case of hierarchical ambiguity, this time embedded in a longer utterance: *The old men and women stayed at home.* It is quite possible that if I say this to an audience, some hearers may conclude that I have referred to old males and all females, others may believe that I have spoken of all old people of both sexes; still others may believe that I am consciously being ambiguous (as I am!) or may simply be unable to decide the matter.

Wells adopts a very simple solution. He assumes that *old men and women* has one or the other hierarchical structure depending on the meaning. Since there are cases in which the hearer cannot tell what the meaning is, this must revert to a consideration of something like 'speaker's intention', which means that the step mentioned earlier is indeed being taken. Before we resort to such a drastic measure, let us look for further evidence.

We might apply the same technique by which we separate homophonous morphemes. *Old men and women* can be substituted for, say, *old people*, in one large set of environments (for example, *old people belong at home* and *old men and women belong at home* are both good English sentences); it can be substituted for *women and old men* in another, partly different, set of environments (*The women and old men stayed at home while the young men went to war; The old men and women . . .*). But *old people* and *women and old men* are not so freely interchangeable. It is by just this procedure that we would, in the first instance, separate *The sun's rays meet* and *The sons raise meat.* Having separated either pair of homophones, we can seek for correlated internal structural differences. In the latter pair, the differences lie in the ultimate constituents; in the former, the only difference is in the hierarchical organization.

But there is a deeper consideration. The hearer, confronted with *The old men and women stayed at home*, is in much the same position as the observer who sees a picture of a hollow cube and can, almost at will, see first one corner and then another as closer to him. There is a third way to see the picture: as an assemblage of straight lines on a flat surface, without depth. This third way is difficult without the special training which artists, whose task it is to produce such visual representations, have to get. The ordinary individual, looking at the three figures A, B, and C, can easily enough see that B is different from both A and C, but he will also see B now as more like A, now as more like C. Since most of our visual experience is with depth perception, we 'read' depth into many two-dimensional portrayals.



A B C

B, then, is ambiguous. An ambiguity cannot exist in the abstract; it must be between things. The alternatives in the visual case are not A and C; they are B's BEING LIKE A or C respectively. 'Being like A' and 'being like C' are two distinct and mutually exclusive matters: B cannot at one and the same time be taken both as like A and as like C.

The same argument holds for hierarchical ambiguity. Our B is *The old men and women stayed at home.* For A and C, we can select respectively *The women and old men stayed at home* and *The old men and old women stayed at home.* B's 'being like' A is one hierarchical structure; B's 'being like' C is another. A hierarchical structure is a class of forms which are alike in a certain way; so is a construction. A single sequence of morphemes cannot be like two mutually exclusive other sequences at one and the same moment for a single hearer, but in some cases it may fluctuate between the two directions of likeness. Interpreting *old men and women* as merely a linear sequence of morphemes is highly unnatural; the linguist, for his special purposes, may have to develop the ability to do so, just as the artist has to develop the ability to see things without depth in order to represent them in such a way that others will report the presence of depth.

Our conclusion, then, is that hierarchical structure is a 'primitive' just as are forms and order. The demonstration turns necessarily on two considerations: (1) there must be many utterances in which the hierarchical structure is unambiguous, to afford a frame of reference; (2) there must be also at least a few in which the hierarchical structure is ambiguous, since otherwise the hierarchical structure would in every case be determined by forms and order, and hence not a 'primitive'.

3.2. One recently proposed type of morphemic analysis turns on the 'primitive' status of hierarchical structure in a way which has never been made clear. This is the utilization of discontinuous morphemes (Harris 1945a). For those to whom this type of analysis appeals, this dependence, to be demonstrated in a moment, may add weight to the desirability of recognizing IC-structure as a 'primitive'. But since there is no situation in which the analyst is actually forced to use discontinuous morphemes, its dependence on IC-structure cannot count in any sense as evidence for the status of the latter.

Consider the following two Latin sentences: (a) *pater bonus filium amat*; (b) *pater bonum filium amat.* The latter, though unusual, was certainly possible; it is easier to handle than more complex and more realistic examples that could be found.

According to the more customary procedure—rejecting discontinuous morphemes—the ultimate constituents of (a) include two occurrences of a morpheme that we may call {nominative} and one occurrence of {accusative}, while those of (b) include one of {nominative} and two of {accusative}.[10] The distribution of the two occurrences of nominative in (a) marks *pater* and *bonus* as going together as partners in a construction, while the distribution of the two

[10] These morphemes, treated as discontinuous or not, are what Harris has called morphemic components, a fact which does not disturb the present argument (see 2.9 (3) of this paper).

occurrences of {accusative} in (b) mark *bonum* and *filium* in the same way. The hierarchical structure in each case is partly marked by the nature and location of these morpheme-occurrences. There is no need, in these examples, for hierarchical structure to be an independent primitive.

Using discontinuous morphemes, the results are different. The ultimate constituents of (a) in this case include ONE occurrence each of {nominative} and of {accusative}—and in (b) there will be found, also, just one occurrence of each of these morphemes. In (a), the one occurrence of {nominative} is an occurrence in discontinuous form, the separated representations coming at the ends of two successive words; in (b) similarly for the {accusative} morpheme.

Now when discontinuous morphemes are recognized, it is necessary not only for the particular shape of each representation of such a morpheme, but also for the number of representations involved in any one occurrence and the precise location of those representations, to be predictable on the morphophonemic level. It is not enough that we be able to predict that if the stem *patr-* and the nominative morpheme co-occur, the total form will be *pater*; we must also be able to predict that if the stem-sequence *patr-* . . . *bon-* . . . and {nominative} co-occur, the total form will be *pater bonus*. Now assume that we have the partial *patr-* . . . *bon-* . . . *fili-* . . . *amat* and one occurrence each of {nominative} and {accusative}. There is absolutely no way of knowing that the total form will be *pater bonus filium amat*, or *pater bonum filium amat*, or possibly something else, unless among the environmentally relevant facts it is given that *patr-* and *bon-* are partners, rather than *bon-* and *fili-*. The distribution of represensations of discontinuous morphemes is not predictable in Latin unless hierarchical structure is allowed to count as part of morphophonemically relevant environment. When we make use of discontinuous morphemes, then Latin sentences (a) and (b), despite overt phonemic differences, are structurally the same as the two English phrases of shape *old men and women*.

The requirement of 2.7 was intentionally phrased broadly enough to allow this. However, it has not been customary to count IC-structure as relevant environment, and it may be that the necessary added complication will be enough to turn some against the whole notion of discontinuous morphemes.

3.3. Next in line is the status of constructions.

Given verb-stem *bake* and the past-tense morpheme, with instructions that they are to go together, the only thing one can do is to put them together as *baked*. There is no problem of hierarchical structure since each constituent is ultimate. Linear order is determined.

Given *John* and *saw* to be put together, the result can be either *John saw* or *saw John* (either of which, partnered by an intonation, might occur as an utterance). Here there is a choice of procedure. If to *John* and *saw* we add a specification as to linear order, then there is no longer any choice between constructions: only *John saw*, which is the actor-action construction, is possible with one order specification, and only *saw John* (action-goal construction) is possible with the other. But instead of approaching the matter in this way, we can equally well regard the order as determined by the construction: *John*, *saw*,

and subject-predicate construction result necessarily in one linear order, the same forms and action-goal construction result necessarily in the other. Whichever course we follow, it is clear that we are confronted with one more variable than the maximum number which can be regarded as independent: forms to be partners, order, and construction cannot in such a case all be independent of each other.

The nearest example to complete independence of these three matters that I have so far discovered is in Chinese. Given *chau*^{3} 'to fry' and *fan*^{4} 'rice', and the specification that the first is to precede the second, there is still a choice of construction—though it does not manifest itself in any overt way within the sequence itself. One possible construction is verb-object, giving *chau*^{3} *fan*^{4} 'to fry rice, fry rice'; the other is attribute-head, giving *chau*^{3} *fan*^{4} 'fried rice'. These are really different: the first is also the construction of *chr*^{1} *fan*^{4} 'to eat rice, eat rice', unambiguously verb-object; the second is also the construction of *hau*^{3} *fan*^{4} 'good rice', which is unambiguously attribute-head.

From here on our argument about *old men and women* applies pari passu to *chau*^{3} *fan*^{4}, and need not be repeated in detail. There are, of course, some larger contexts in which *chau*^{3} *fan*^{4} is marked unambiguously as one or the other construction, just as there are larger environments in which the hierarchical structure of *old men and women* is not ambiguous. But there are also larger contexts in which the ambiguity remains. The construction, then, cannot be derived from the forms and the order. The reverse is possible: given forms and construction, the order can be regarded as determined—it just so happens that either of two constructions, with the same two constituents, results in the same order.

The conclusion to which we are forced is that, at least in some cases in some languages, choice of construction is a primitive.

3.4. If a particular version of IA allows the recognition of marked constructions, then the question as to the primitive or derived status of constructions becomes trivial: at least such constructions which have markers must have primitive status. Consider *John and Bill*, a bipartite construction with marker *and*. This form contrasts with *Bill and John* only as to order, with *John and Mary* (or *Tom and Mary*) only as to form, and with *John or Bill* only as to construction—since *or* is the marker of a different construction. In this case, all three factors are independent. It immediately follows that in *old men and women* all four factors are independent, since in addition to the *X and Y* construction there is also the matter of hierarchical structure, already demonstrably independent of the other factors.

3.5. So the four factors, the status of which we set out to survey, all have to be recognized as potentially independent, and therefore as primitives. If there is a possible exception, it is, surprisingly enough, linear order—one of the two factors (the other being forms) which would seem so obviously primary. The only situation discussed above in which linear order had to accepted as independent of all other factors was in the case of marked constructions like *John and Bill*, and it is possible to proscribe marked constructions altogether.

The survey also shows, however, that in the bulk of cases not all of this machinery is needed. In most cases, a determination of two or three of the factors leaves no choice for the remainder. It is this fact which gives rise to the most embarrassing tactical trouble inherent in IA: machinery which has to be in our workshop for use in certain marginal cases tends to obtrude itself where it isn't wanted.

Thus Bloch writes as follows (1947, p. 400): "the preterit form *waited* . . . can be described as follows. It consists of two morphemes, /weyt/ and /ed/, occurring in that order. The meaning of the first morpheme is a particular action that we need not specifically define here; that of the second is 'past time' or the like. The constructional meaning of the order in which the two morphemes occur is approximately 'perform a certain action at a certain time'." Bloch has cut down on the total amount of machinery by identifying 'construction' and 'order' (which, as we have seen, is in general questionable). But, still, entities are multiplied beyond necessity. Given the morphemes /weyt/ and /ed/ to be put together, actually nothing further need be said at all on the tactical level. There is no possible linear order save that of /weyt/ first and /ed/ second. Nor—if we separate order and construction—is there any choice of construction. Semantically, it is quite pointless to break down the meaning of the whole form *waited* into THREE parts: structurally, there are two and only two independent variables, and the only valid procedure is to assign, as the meaning of the second variable, everything which differentiates the meaning of *waited* from that of *wait*.

IV. Morphophonemic Troubles With IA

4.1. Most morphophonemic problems find simple answers in IA, for there is available a wide variety of morphophonemic techniques all well within the bounds of the IA model. However, there is a refractory residue, troublesome not because no solution can be found, but either because a multiplicity of solutions present themselves, no one seeming much better than another, or because the intuitively best solution is clearly in violation of the fundamental orientation of IA. This residue includes such cases as *took*, *put* (past tense), *children*, French *bon* and *bonne*, Chinese *lya*^{3} and *lyang*^{3} 'two'. We need deal in detail only with one of these, and *took* will do.

4.2. The following morphophonemic solutions have been proposed (or might be proposed) for *took* (we here follow, in the main, Bloch 1947, pp. 400–1):

(1) *took* is a single morpheme, so that there is no morphophonemic problem.

(2) *took* is a portmanteau representation of the two-morpheme sequence *take* and /ed/.

(3) *took* is an allomorph of the morpheme which appears elsewhere as *take*, plus a zero allomorph of /ed/.

(4) *took* is a discontinuous allomorph /t . . . k/ of *take*, and an infixed allomorph /u/ of /ed/.

(5) *took* is *take* plus a REPLACIVE MORPH /u/ ← /ey/ (read '/u/ replaces /ey/').

Let us consider these one by one.

(1) is unacceptable because it controverts the tactical parallelism between *took* and *baked* and many other obviously composite forms.

(2) is the most general solution, since it avoids, in a sense, the problem of identification of partial similarity in shape. But this very avoidance is arbitrary: *took* and *take* are partly similar in phonemic shape just as are *baked* and *bake*, and similar in meaning also in the same way; the fact should not be obscured.

(3) is arbitrary because it assigns to something that isn't there phonemically —something that is 'there' only in the fact that it calls for a phonemically distinct allomorph of the accompanying morpheme—a meaning which would more naturally be assigned to some location within the overt form *took*. The same solution for the past tense *put* (or the plural *sheep*) is even more strained since there is not even this phonemic effect on the accompanying morpheme. Within the same special rules that allow (3), it would also be possible to propose that *took* be interpreted as an allomorph of /ed/ accompanied by a zero allomorph of *take*. I see no reason why (3) should be considered any more or less seriously than this alternative. (3), in effect, gains nothing at all over (2).

(4) is in some ways the most attractive (with some manipulation, this proposed solution can be derived from Nida's discussions in 1948, 1949; his 'replacive' terminology tends to obscure the matter). It meets the objections so far raised to the other alternatives, as well as that which will shortly be raised to (5). The most serious criticism so far offered is that we might be led to interpret *take*, also, as an allomorph /t . . . k/ plus an infixed allomorph /ey/; this is not justified because *take*, like *bake*, *rake*, *catch*, and so on, acts tactically like a single morpheme. This is not a very serious criticism, since we need not be so submissive to the proposed analogy. Within the IA framework, (4) is probably the best answer.

(5) is not valid within IA. It calls an 'allomorph' something which is not in conformity with the general definition of the term. A morph is composed wholly of phonemic material or (as the limiting case) of no phonemic material at all. A 'replacive', like '/u/ ← /ey/', is not by any stretch of the imagination composed of phonemic material.

The same comment applies to 'subtractives', which happen not to apply in the case of *take*. It is tactically convenient to regard French *bon* as more complex than *bonne*, Chinese *lya*^{3} 'two' as more complex than *lyang*^{3} 'two'.[11] *Bon* is then, it is proposed, to be taken as *bonne* plus a 'subtractive' morph consisting of loss of final consonant (and nasalization of the vowel); *lya*^{3} is to be taken as *lyang*^{3} plus a terminal 'subtractive'. A subtraction is no more composed of phonemic material than is a replacement.

4.3. Why should so much emphasis be placed on this? Could we not modify our definition of 'morph' in such a way as to allow subtractives and replacives in those circumstances where they seem so clearly convenient? Of course we

[11] This comment on 'replacives' and 'subtractives' is given by Bloch in fn. 3 of Nida 1948. The error discussed here crops up also in Hockett 1947b, pp. 339–40; Hockett 1950, p. 82–3; Harris 1942, p. 170–1.

can do so. But such action seems to be equivalent—perhaps rather unexpectedly—of removing the keystone of the whole IA arch; the model begins to collapse. All the criteria which have been so laboriously established as a basis for the selection of one morphophonemic treatment or another have to be supplemented by new clauses. Even the tactical picture tends to be modified. When we pick up the pieces and try to fit them together again—without restoring the keystone—we find that we are no longer dealing with anything that looks like IA; we have a new model on our hands. After a short intermission this new model will be put on exhibit.

V. Mathematical Interlude

5.1. Mathematics is a good place to turn to for analogs of structures, since mathematicians and mathematical logicians have as their business the construction of structures of the greatest variety and generality. For our purposes, we shall only have to venture into the most elementary phases of their activity (see the more elementary and readable portions of Quine 1940, Birkhoff and MacLane 1944).

A good many MATHEMATICAL SYSTEMS are characterizable wholly or primarily as consisting of a set of ELEMENTS for which certain RELATIONS are defined. One such system has as elements all the positive integers, and as one elementary relation, the relation of 'greater than': two is greater than one, five is greater than two, and so on. A relation ties together pairs of elements, or, in some cases (for example, the relation 'is between . . . and . . .'), more than two elements. A relation is also quite satisfactorily definable as a CLASS OF ORDERED PAIRS (or of ordered n-ads with n greater than 2) OF ELEMENTS. In this sense, the number-pairs (3, 2), (5, 1), (2, 1) are said to be members of the relation 'greater than', while the number-pair (1, 3) or (1, 1) is not. The two approaches to relations are equivalent: we can begin with some property such as 'greater than' and observe that some pairs of numbers bear this relation, or we can begin with any set of ordered pairs of numbers—selected, it may be, quite randomly—and define a relation simply by listing the ordered pairs that belong to it.

Now it is easy to show that a construction, in the linguistic sense (within IA), is a relation (this proposal has not been made in print, to my knowledge; Wells has incorporated it in an as yet unpublished report on linguistics and semantics). The constituents of a constitute are elements; they are in a particular arrangement (linear order). A construction, then, is a class of ordered n-ads of constituents, whereupon a construction is a relation.

There is one point at which this characterization seems (wrongly) to fall down. Two and three are numbers; 'three is greater than two' is not a number, nor is (3, 2) a number. *Black* and *cat* are forms; and *black cat* is a form too. It makes sense to say that *black cat* participates, as a whole, as an element in larger constructions; it certainly does not make sense to say that (3, 2) is greater than something or less than something. This apparent discrepancy is due to the perhaps overly simple nature of the particular mathematical system we have chosen. It is hardly possible to say that (3, 2) can be greater than some

number, but it is perfectly possible to define—in the arbitrary way mentioned above—a relation which has as members not only ordered pairs of numbers such as (3, 2) and (3, 1) but also ordered pairs of elements one or both of which are in turn ordered pairs of numbers—such as ((3, 2), 1) or (3, (2, 1)), or even ((3, 2), (3, 1)). Most such invented relations would be trivial and uninteresting, but some might be fruitful, and all are perfectly acceptable logically. So there is nothing to keep us from recognizing a relation in linguistics which has as members both such ordered pairs as *black cat* and also such as *big* | *black cat*.

5.2. Another great class of mathematical systems are characterizable as consisting of a set of elements for which certain OPERATIONS are defined. One such system has, as did our exemplification of a system with relations, the positive integers as its elements, and has, as one elementary operation, that of ADDITION. Addition applies in the first instance to pairs of positive integers, and is therefore a BINARY operation: two plus two is four, three plus four is seven, and so on. Addition has certain further properties. For instance, it is COMMUTATIVE: we get the same result adding two and three whether we start with the two or with the three. There are operations which are not commutative; e.g., subtraction defined over the same set of elements, since five minus seven is not only not the same as seven minus five, but actually meaningless. Addition is also ASSOCIATIVE: $(2 + 3) + 6$ and $2 + (3 + 6)$ add up to the same total. By virtue of this property, parentheses can be dropped and the operation takes on the appearance of not being binary at all, but of applying to any number of terms, two or more. Fundamentally, however, it is convenient to regard addition as binary.

It can be shown that operations are reducible to relations, so far as their logical status is concerned. Thus the binary operation of addition, by which $2 + 3 = 5$, can also be interpreted as a ternary relation holding between the ordered triad of numbers (2, 3, 5) and between any ordered triad of numbers (a, b, c) for which it is true that $a + b = c$. In general, any n-ary operation is logically reducible to an appropriate $(n + 1)$-ary relation.

Psychologically, however, this replacement leaves something out. Relations seem static, whereas operations seem dynamic—seem to generate something which we perhaps did not know was there. One use of mathematical systems is in computing; that is, in discovering implications of assumptions that are not known, or not obvious, to the computer until the computation has been performed. Computing makes use of systems involving operations, not those characterized purely by relations, underscoring the importance of the dynamic or generative nature of operations. Logical identifications are always achieved by leaving something out; the mathematical logician is willing to leave out this psychological difference between relations and operations, but the ordinary mathematician is not. For our purposes, we shall follow the inclination of the ordinary mathematician.

5.3. There are two features of operations which may render them useful to us in linguistics, in place of relations—despite the logical substitutability which has been demonstrated.

One of these is the fact that the result of applying an operation to a pair (or set) of elements, or to a single element, is also an element. 'Two is greater than one', whatever it may be, is certainly not the same kind of element as are two and one. But two plus three is the same kind of element as are two and three; indeed, it is specifically that element, another name for which is 'five'. So if we say that *black* and *cat* are joined by an operation, instead of by a relation, we shall be more comfortable about calling *black cat* a form too.

The other is the fact that there are singulary operations, but no singulary relations (not 'unitary', 'uninary', or 'unary'; see Quine 1940, 13, fn. 1). We see this from the logical substitutability of operations and relations. An n-ary operation is equivalent to an $(n + 1)$-ary relation, so that the obvious existence of binary relations guarantees equally obvious singulary operations; but it there were singulary relations there would have to be nullary operations, and the one is as meaningless as the other.[12]

An example of a singulary operation in mathematics is 'reciprocal of' or 'negative of'. The latter is undefined for positive integers, but is defined for the set of all integers, positive, negative, and zero: the negative of three is minus three; the negative of minus four is four; the negative of zero is zero. For that matter, given a binary operation, any number of singulary operations can be devised. If addition is a binary operation, then addition of two to, addition of three to, and so on, are all singulary operations.

The utility of a singulary operation for linguistic analysis can be demonstrated with a form like *waited* where, as we saw in 3.5, it is pointless to isolate more than two factors. It may be advantageous to interpret *waited* as the result of a singulary operation applied to *wait*, at least in that this gives us just two separate factors.

5.4. If 'relation' is our closest analog to 'construction', then certainly the linguistic analog to 'operation' is 'process'. A grammatical model constructed in terms of this analogy ought to differ from IA just as operations differ from relations: it should be dynamic instead of static. It was pointed out in 1.2–4 that this was one of the chief characteristics of the older unformalized IP model, and we shall see that it is retained in the formal version about to be presented.

VI. Item and Process

6.0. The statements which follow parallel, as much as possible, those of II, and are abbreviated where reference to II can easily serve to fill them in; examples are left for VII.

6.1. A linguistic form is either SIMPLE or DERIVED.

6.2. A simple form is a ROOT.[13]

6.3. A derived form consists of one or more UNDERLYING FORMS to which a

[12] Since a relation is a class of ordered n-ads, the singulary analog is a class of ordered monads; i.e., simply a class. What a nullary operation would be still escapes me entirely.

[13] The choice of terminology is difficult here; I do not recommend continued use of 'root' and terms stemming from it. 'Morpheme' would be preferable, but is avoided here in order better to contrast the two models.

PROCESS has been applied. The underlying forms and the process all recur (save for occasional uniquenesses) in other forms. The underlying form or forms is (or are) the IMMEDIATE CONSTITUENT(S) of the derived form, which is also called a CONSTITUTE; each underlying form is said to occupy a given POSITION; each, if there are more than one, is the PARTNER of the rest.

6.4. Some of the phonemic material in a derived form may be, not part of any underlying form, but rather a REPRESENTATION or MARKER of the process. (Such markers are not roots.)

As in the parallel statement for IA (2.4), this necessitates a statement of conditions; we defer this to VII.

6.5. The TACTICAL PATTERN of a language is completely covered by a set of statements of the following form:

(1) A list of the processes.
(2) Under each process as heading,
  (2.1) Enumeration of the position or positions involved.
  (2.2) For each position,
    (2.21) A list of the roots which occur in that position, and
    (2.22) A list of the processes which produce forms which occur in that position.

There is here no analog to statement (2.2) of section 2.5.

6.6. A root may appear in more than a single phonemic shape. A single shape of a root is a ROOT-ALTERNANT.

A process may have more than one representation. A single representation of a process is a marker. A marker consists of the difference between the phonemic shape of a derived form and the phonemic shape(s) of the underlying form or forms. That is, a marker may consist of phonemic material in some specific position relative to the phonemic material which is identical with that of the underlying form, or it may consist of something present in the derived form in place of something else present in the underlying form, etc.

6.7. The alternations in shape of roots, and the choice in a particular instance of one or another marker of a process, are predictable in terms of the environments in which they occur (plus, of course, one's morphophonemic statements).

'Environment' is definable as in 2.7.

6.8. All the phonemic material in an utterance is accountable for in terms of the roots which occur in the utterance and the processes to which they have been subjected.

6.9. Empty root-alternants, portmanteau root-alternants, and links are definable and allowable as in IA, should there be any need for them. Zero alternant roots and zero markers of processes are likewise allowable, under similar limitations.

6.10. The statement of shapes, alternations, and conditions of alternation describes the morphophonemic pattern of the language; further considerations remain as in IA (2.11).

VII. Comparisons

7.1. First we give examples of IP treatment.

(1) *Baked* is a derived form, with a single immediate constituent *bake*, which happens to be a root, subjected to a singulary process which we can simply label PAST-TENSE FORMATION. This process has various markers; when applied to *bake*, the marker is a /t/ which follows the phonemes of the underlying form.

(2) *Took* is tactically like *baked*, with underlying form *take*. The morphophonemic difference is that here the singulary process in question has a marker consisting of replacement of the stem vowel /ey/ of the underlying form by /u/.

(3) *John* and *saw*, subjected to the binary process of PREDICATION, give *John saw*, necessarily with that order, and with zero marker unless the order be taken as a marker. The same forms, subjected to the binary process of RESOLUTION, give *saw John*, again with order determined and zero marker. As with IA, we could alternatively take the order as primary and the process as determined. In support of the first alternative, we can present comparable cases in mathematics. If we are concerned with the positive integers only, then the operation of subtraction, if it is to apply to seven and three, necessarily requires the order $7-3$, since $3-7$ is undefined.

(4) Chinese *chau*^{3} and *fan*^{4}, subjected to either the binary process of resolution or the binary process of MODIFICATION, give *chau*^{3} *fan*^{4}, respectively 'fry rice' and 'fried rice'. The difference in positions of occurrence in larger forms, and the difference in meaning, correlate with the difference in process.

(5) *John and Bill* is (perhaps) underlying forms *John* and *Bill*, subjected to the process of ADDITION, for which the only marker is *and*. *Bill and John* is a different derived form, involving the same underlying forms and the same process, but a different order—order in this case being separable from process. Note that this process, here called 'addition', does not have the properties of the mathematical operation of the same name; grammatical addition is not commutative. Indeed, probably no grammatical processes will be found to be commutative.[14]

7.2. Our IP model differs, at least superficially, from anything to be found in the writings of Sapir.

This is partly because we have tried to incorporate into the more formalized version of IP some of the results of recent investigations carried on within the

[14] The inventors of mathematics, as speaking humans, distill mathematical notions out of the raw-material of everyday language. Their notions are derived by leaving something out of the nearest everyday-language analogs. A linguist analyzing English must assume that *two plus three* and *three plus two* are different forms. The mathematician chooses to ignore everything which differentiates the meanings of these two forms, and by so doing, he renders his 'addition' commutative. It may seem strange that we who are concerned with the total complexity of language should turn to mathematics for help, considering the ultimate source of mathematics. But the circle is not in fact closed: the mathematician derives his notions by abstraction from language, whereas we are deriving, not language itself, but a WAY OF HANDLING language, from mathematics.

IA framework. Mainly this means the contrast between tactics and morphophonemics, which does not emerge at all clearly either in Sapir's writings or in Bloomfield's 1933 discussion. It would be a loss of ground to expunge this contrast during the reconstruction of IP.

But its retention leads us to use the term 'process' in a way different from Sapir's use. For Sapir, such matters as vowel-change, suffixation, reduplication, and the like, were 'processes'. For us, 'vowel-change' refers to one possible canonical form of marker, a canonical form represented both in *men*, from *man*, and in *took*, from *take*, though the markers in *men* and *took* represent tactically different processes in our sense of the word; the marker in *took* and the marker in *baked*, representing the same process in our sense, are of different canonical forms and thus would be different 'processes' in the Sapir sense. Nida's (1946) earlier contrast (derived closely from Sapir) between 'phonological process' and 'morphological process' approaches closely our contrast between canonical forms of markers, on the one hand, and tactically relevant processes on the other.

All of this would be easier to say, and at the same time perhaps less necessary, had we abandoned the term 'process' and simply imported 'operation' from mathematics, or had we used such a pair of terms as 'processeme' (tactical) and 'alloprocess' (morphophonemic), as a direct splitting-up of Sapir's more nebulous notion of 'process' in the light of recent developments.

7.3. The examples of 7.1 show that, by and large, grammatical descriptions cast in IP will run parallel to those cast in IA. There will be slight differences in terminology, and the wording certainly gives a 'dynamic' rather than a 'static' feel to the statements. Apart from these matters, the main differences will be marginal—though perhaps crucial.

IP obviates the major tactical and the major morphophonemic difficulty of IA. We are not confronted with superfluous machinery in the case of *baked* or *took*: the process involved is singulary, so that the only factors are the respective underlying forms and the process. The morphophonemic difficulty which IA gets into with *took* is obviated, since the whole frame of reference is one in which the difference between *took* and *take* is just as acceptable a process-marker as that between *baked* and *bake*.

On the other hand, IP makes for certain difficulties avoided by IA.

In the first place, a 'pure' IA approach (which bars the interpretation of any morphemes as markers of constructions) is clearly much more homogeneous than either a less pure IA, or IP. This homogeneity is not as simple as has been thought; 'items', true enough, are either morphemes or sequences of morphemes, but still one has to contend with the independent status of order, constructions, and hierarchical structure. Even so, there is a clear difference between taking some phonemic material as 'root' (=item) and some as 'marker' of processes, as IP requires, and the simple procedure of taking all phonemic material as either morphemic or else morphemically irrelevant and morphophonemically predictable.

The problem of priority, evaded by IA, comes back into the picture. How are we to tell under what conditions to interpret a derived form as involving

two or more underlying forms and a binary or higher-order process, and under what conditions to interpret it as involving a single underlying form and a singulary process. In one sense, of course, this is an extension to the case of two versus one of a problem found in IA: when do we recognize three ICs rather than two, or, in general, n rather than $n-1$?

The answer probably lies partly in the cases which are solved more easily by IP than by IA. *Baked* and *took* are interpreted in terms of a singulary process because to do otherwise leads to tactical and semantic trouble in both cases, and to morphophonemic trouble in the second. Obviously cases will be found more difficult than these, and a full elaboration of the necessary criteria is not to be expected overnight. In this very connection, IP is sure to encounter its refractory residue, different in content from, but similar in implications to, that of IA.

7.4. Another contrast between IA and IP turns on the number of constructions one has to recognize for a language under IA, versus the number of processes necessary for the same language under IP. The criterion of economy seems never to be evoked in IA so far as number of morphemes is concerned: there are in any case a great many morphemes, and a few more or less hardly matters. But economy enters the picture in reducing the number of classes of morphemes, or the number of major form-classes, or constructions, as much as possible. This is why, when confronted with the doublet plurals *brothers* and *brethren*, we are willing either to recognize two homophonous stem-morphemes *brother* requiring different allomorphs of the noun-plural morpheme (Hockett 1947b, p. 330), or, if needs be, to recognize that the plural element in *brethren* is, after all, a different morpheme from that in *brothers* (Ibid., and Nida 1948, p. 415, by implication). It hardly occurs to us to suspect that the constituents are the same but the construction different.

The same tendency also manifests itself in the effort sometimes made to reduce, if not the total number of constructions, at least the total number of CONSTRUCTION-TYPES—that is, classes of constructions which manifest some similarity of behavior. Thus in Chinese there is no way in which one can call *hau*^{3} *fan*^{4} 'good food' (stative verb followed by noun) and *ye*^{3} *lai*^{2} 'also come' (adverb followed by verb) instances of a single construction; their privileges of occurrence in larger stretches of speech are quite different. But one can say that the two constructions exemplified by these forms are, in their turn, instances of a single construction-type, sharing the features of endocentricity and of head second rather than first. Without much bother, one can establish a classification of all the constructions in Chinese into eight construction-types. To do so is esthetically satisfying, probably just because of the psychological impact of the criterion of economy; in addition, it affords a useful basis for the organization of tactical statements about Chinese.

Perhaps it does not matter, but if we follow IP we are going to have to sacrifice some of this partly covert drive towards one form of 'economy'. The number of processes in a language, under IP, will probably in general be greater than the number of constructions in the same language under IA. For *brothers* and *breth-*

ren, the interpretation of two homophonous underlying forms is still open to us, but loses much of its attractiveness. There is only one alternative: a single underlying form (*brother*) with two different singulary processes. And while the singulary process involved in *brothers* (normal noun-plural formation) is one which recurs widely, that in *brethren*, since it contrasts with that in *brothers*, is presumably also different from the one in *boys*, *men*, *taxes*, and so on; it is as forlorn as a process as the first IC (under either IA or IP) of *cranberry* is forlorn as an item. Uniquenesses under IP, in other words, may be unique constituents or unique processes. The only possible way to appease our parsimoniousness is to devise PROCESS-TYPES on the cross-model analogy of construction-types.

7.5. Whether a grammatical description of a language is satisfactory or not depends in part on the use we want to make of it. Quite apart from esthetic or stylistic considerations, which can and do vary from one reader of grammars to another, and setting aside such matters as application to language pedagogy, there remain a number of properties which a grammatical description must have if it is to satisfy us for any scientific purpose. These properties have already been hinted at (particularly in 3.0). A grammatical description must be a guidebook for the analysis of material in the language—both material examined by the analyst before the description was formulated, and material observed after that. Lexically the coverage need not be complete, since new morphemes can turn up as one continues to record the language. Otherwise it must be complete in the sense just described. The description must also be prescriptive, not of course in the Fidditch sense, but in the sense that by following the statements one must be able to generate any number of utterances in the language, above and beyond those observed in advance by the analyst—new utterances most, if not all, of which will pass the test of casual acceptance by a native speaker.

If these are criteria for the evaluation of a grammatical description, what we need for the evaluation of models is a set of appropriately related metacriteria. The following list is perhaps not complete, and I am hesitant about the fifth, but no set would be complete which did not include the first four:

(1) A model must be GENERAL: it must be applicable to any language, not just to languages of certain types.

(2) A model must be SPECIFIC: when applied to a given language, the results must be determined wholly by the nature of the model and the nature of the language, not at all by the whim of the analyst. It is not lack of specificity if the model requires us to subsume certain facts more than once, from different angles; it would be a lack of specificity if the model ALLOWED us to take our choice in such cases, instead of FORCING one choice or other or their joint option.

(3) A model must be INCLUSIVE: when applied to a given language, the results must cover all the observed data and, by implication, at least a very high percentage of all the not-yet-observed data. This is the analog of the 'guidebook' criterion (not metacriterion) mentioned earlier.

(4) A model must be PRODUCTIVE: when applied to a given language, the

results must make possible the creation of an indefinite number of valid new utterances. This is the analog of the 'prescriptive' criterion for descriptions.

(5) A model must be EFFICIENT: its application to any given language should achieve the necessary results with a minimum of machinery.

If we were confronted with two models, one of which fulfilled all the above requirements while the other did not, choice would be easy. If we were confronted with two, both of which fulfilled all the requirements, we would have to conclude that they differed only stylistically. Neither of these situations, of course, is at present the case. Neither any existing version of IA nor any existing version of IP meets all the metacriteria. Insofar as such matters can be felt quantitatively, it seems to me that IP, as constructed here, comes at least as close to satisfying the requirements as does IA, though perhaps no closer. In other words, what we have is two main types of model, neither completely satisfactory. Our course in this case is also clear. We must have more experimentation, as much with one model as with the other—and with the devising of further models too, for that matter—looking towards an eventual reintegration into a single more nearly satisfactory model, but not forcing that reintegration until we are really ready for it.

Cornell University

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MEANING AND USE

RULON WELLS

1. Russell and Wittgenstein. Many ideas stemming from Bertrand Russell are of great value for linguistics; then why has their diffusion hardly begun to take place? Less from sheer ignorance of the ideas than from unawareness that they are relevant. If a popular cliché can be trusted, technical results need to be 'translated' in order to become available to the layman. But here it's not hard words that are the obstacle; in the last analysis it is the very peculiar relationship between science and philosophy.

In the present paper we shall have under consideration a body of writings by men who have aimed at philosophy but have contributed to science en the way. From their own point of view their contributions to science are incidental. But our point of view is the reverse of theirs. We are called upon, not merely to excerpt their ideas but to adapt them. A subtle difference?—but big enough to have preserved the said ideas nearly intact. A chemical analogy suggests itself in preference to a mechanical one. Philosophical and scientific aspects of their work are chemically combined, not mechanically mixed; no mere screen, no dipper, no magnet will separate them. I hope that my meaning will become clearer later on than it can be here.

Within the Russellian movement three reasonably distinct main branches may be noted. In order of their appearance they are: (1) The approach of Russell himself, formulated in the first quarter of this century. (2) Logical positivism, deeply indebted to Wittgenstein's *Tractatus Logico-Philosophicus* (1922) and taking shape as a self-conscious, self-named group a little before 1930. (3) A movement centering about Wittgenstein after he had accepted a call to Cambridge in 1929 and had rejected many of his own earlier doctrines. (As early as 1914 Russell began acknowledging "the benefit of vitally important discoveries, not yet published, by my friend Mr. Ludwig Wittgenstein"—(1914), p. vii. Thus Wittgenstein has played a major part in all three branches of the Russellian movement.) As Wittgenstein published practically nothing in this period, his ideas were disseminated—apart from his personal teaching—only through the writings of his disciples, John Wisdom and others, and through the private circulation of various unpublished lecture notes.

After the Second World War leadership in the Wittgenstein movement, and thereby in British philosophy in general, shifted to Oxford, where a remarkable quickening had taken place. Ideas of Wittgenstein's were joined there to the somewhat divergent ideas of Gilbert Ryle and others, and a group of thinkers with sufficient consensus to warrant being called a school has emerged. The name that has become current for these Oxford philosophers and for others, wherever situated, of closely related views, is 'the philosophy of ordinary language'. There are writers such as John Wisdom and C. L. Stevenson, considerably influenced by Wittgenstein but to whose thought the term 'philosophy of ordinary language',

in the technical sense that is in process of growing up, might not be applied. I will bypass such questions of detail by speaking, usually, of 'Wittgensteinians'.

Wittgenstein retired from Cambridge in 1949 and died in 1951. *Philosophical Investigations* was published in 1953. For obituary notices see Anonymous (1951) and Ryle (1951a); for reminiscences see Russell (1951), Wisdom (1952), and Moore (1954). Ryle (1951b) is a popular discussion. Journal publications by Wittgensteinians are principally to be found in the volumes for the past decade of *Mind*, *Analysis*, *Proceedings of the Aristotelian Society*, *Aristotelian Society Supplementary Volumes*, and *Philosophical Review*.

In what follows I shall give few direct quotations. References are made less for the assignment of credit than to guide the interested reader. The 'chemical' job of isolating the scientifically interesting aspects of the movement and showing how they might be developed is so intricate that it is not worth while to retrace the steps here. For present purposes, it is only the end result that matters.

The whole Russellian movement will not be under discussion, but (primarily) only the third or later-Wittgenstein branch. Since this is the most recently developed branch, and for that very reason the least widely known, the decision to concentrate on it seems reasonable. But the choice does require us, for example, to pass over the important Chapter VII, "Analysis of conversational language", of Reichenbach (1947), which belongs to the second or logical positivist branch.

2. The dyadic conception of meaning. The best way to bring out Wittgenstein's novelty is by comparisons. Beginning with a half-historical survey of varying conceptions of the sign, we will move on (Sections 3 and 4) to Bloomfield's view of meaning, which is superficially similar to Wittgenstein's but significantly different, and will end the preliminaries with some account (Section 5) of Russell's program and his solutions.

The most obvious way to conceive meaning is to conceive it as a dyadic relation between a sign and an object. So natural, so appealing, this conception has cropped up again and again. Even sophisticated thinkers have bent their ingenious efforts to preserving it. A rival account, and one that is rather popular today, recognizes three factors: sign, object, and user. But a moment's thought will prove that even this triadic conception is not exact. For instance, we would need to mention the time of signification: A is a sign of B to C at time D; at another time, it may be a sign to B of something different, or may not be a sign to B at all. Further, this triadic conception could be modified by speaking not (with Morris) of the user, but (with Peirce) of the interpretant, that is, of the thought or mental effect *in* the interpreter or user. If we do this, and consider interpretants as individual acts, then the time reference is already implicit and it would be redundant to mention it again. If we consider interpretants as kinds of acts, then the time must be specified. Thus there can be different versions of the triadic conception, some more exact than others.

The modern triadic conception has been given two applications. First, it underlies the argument for the view that 'the linguistic sign is arbitrary'. Consequently, the moderns who accept it are taking sides on the ancient *physei–thesei* controversy. Second, and in consequence of the first, it has been used

(above all by C. K. Ogden) to refute word magic, i.e. the doctrine of an intrinsic identity between linguistic sign and its object. For if there is no intrinsic connection at all, there cannot be an intrinsic connection of identity.

Ogden and Richards—to this extent resembling Aristotle—analyze the triadic relation as a conjunction of two dyads: one holding between sign and interpretant, the other between interpretant and object. This analysis enables them, or at least encourages them, to treat the resulting dyads as varieties of the cause-effect or stimulus-response relation. Odgen and Richards are closer to Bloomfield's position than he (1933, p. 515) gives them credit for.

The preceding paragraphs have been arranged around the problem, What variables or factors does signification involve? The only one on which there has been agreement is the sign (vehicle, *signifiant*) itself. As to the nature of the object, opinions have been too numerous to mention. Everyone recognizes that one and the same sign may have more than one sense; and nominalistic philosophers hold that every fresh instance of a sign involves a new meaning of it. But leaving aside both these considerations, there is the distinction of denotation and connotation to reckon with. We can prove rigorously that *some* such distinction is needed; for there are pairs of expressions that apply to the same objects but that differ in objective meaning somehow or other. Besides, "No one with a respect for sense would dream of pointing to someone . . . and saying . . .'the meaning of yonder phrase is suffering from influenza' " (Ryle 1932, p. 25; cf. 1949, pp. 70, 73–4). Quine in his address to linguists (1953, p. 47; cf. 9, 21, and 130) makes the point by distinguishing meaning from reference. And years ago Gardiner stressed the distinction of meaning from thing-meant (Ullmann 1951, pp. 33fn., 70). The simple dyadic conception of meaning leaves this distinction between different kinds of 'objects' out of account just as much as it leaves the interpreter.

The number of theories of meaning is vast; so is their variety. Semanticists have been groping in a labyrinth. But there is a cheering thought. To get out of the maze is not our sole wish; we want to *explore* the maze too. We want to understand it, become intimately familiar with it, know all its ins and outs. What we would welcome is not so much a thread of Ariadne as a flashlight.

Let us feel around in one of the darkest alleys again, the dyadic theory of meaning. One kind of sign, the symbol in one sense of the word, is a surrogate for its object. The conscious dreaming of a journey substitutes for the unconscious thought of death; the mathematician's square-root sign stands for the words 'that (positive) number whose square is ____'. Some philosophers have tried to handle all signification by bringing it under the idea of surrogation. Another version of the dyadic, single-object conception tries to assimilate all meaning or signifying to naming. Russell tried to do this too, and his and Wittgenstein's big discoveries came from their perception of the difficulties. If we hold that all expressions are really names, then the first difficulty is that we must explain why some expressions are obviously names whereas others appear not to be. A century ago people were willing to say that whereas 'John' names a person, 'or' and 'not' name thoughts in the mind. But this half-hearted view opens up a new

approach. Why not unify our account by making *all* words name something mental? To avoid this absurdity Russell (1940) introduces a distinction between two *kinds* of meaning: expression and indication. Every sign that indicates, also expresses; but some signs that express don't indicate anything. Logical words like 'or' and 'not' express a state of mind but do not indicate any objective state of affairs, unless a state of mind itself is being talked about. Since indication is virtually the same as the old relation of naming, Russell has abandoned the dyadic single-object doctrine by recognizing that a sign may have two 'objects' simultaneously—that which it indicates and that which it expresses.

Now although what a sign indicates corresponds to what it denotes, what it expresses does not coincide with what it connotes. The reason is that connotation as logicians understand it is something objective, not something mental. Or at least this is what one school of thought maintains. Expression, on the other hand, is more like the interpretant of sign–object–interpretant theories.

Here we have an example of the difference between philosophy and science. The philosopher ponders such questions as whether we need to recognize extramental objective connotations or whether we can consider that they are abstractions created by the mind and existing only in it. The scientist on the other hand would be only too glad to hurry right past this question, if someone can show him how to do it. This problem of autonomy will occupy us in the next section. Here it may be remarked that Wittgenstein's slogan, 'Don't look for the meaning, look for the use', virtually offers the scientist the help he is looking for.

3. Bloomfield and autonomy. Among the notable and distinctive features of science—and one that sets it in contrast with philosophy—is its policy, 'Divide and conquer'. The piecemeal method (Russell) of considering problems in isolation is one secret of its success; but it is a policy that only makes sense as a deduction from a higher one, viz. the overarching conception of scientific results as tentative.

Wherever possible, we would like to apply the 'Divide and conquer' policy in the following way. We would like the several sciences to be so related that each one shall be able to profit by relevant advances in the others, and yet not be held back by their difficulties. This scheme for letting a science eat its cake and have it too can be put into practice to whatever extent we can establish a twofold relationship: (i) autonomy and (ii) reducibility.

(i) The concepts and propositions of one field are autonomous from those of another when changes in the latter do not require changes in the former. Autonomy of A from B is the more advisable, the more likely it is that B will undergo change.

(ii) The concepts and propositions of one field are reducible to those of another if they can be reinterpreted, in a regular and systematic manner, as concepts and propositions of the latter. Descartes's analytic geometry reduces geometry to algebra; Whitehead and Russell's *Principia Mathematica* reduces arithmetic and algebra to the logic of classes and relations. Instances from empirical science have only recently been worked out. A number of them are very beautifully discussed in Woodger (1952).

(Reducibility is what the logical positivists have called 'unity of laws', in contrast to the weaker relationship of 'unity of language'. See papers by Feigl and by Carnap in Feigl-Sellars, 1949, pp. 22–3, 421–2, and 510–4. We might say that reducibility of A to B permits A to be 'hooked up' or 'plugged into' B, and autonomy from B permits A to be 'unhooked' or 'unplugged'.)

These abstract remarks can be immediately applied to semantics. The science of psychology and the non-scientific field of philosophy are the two areas or inquiries upon which it is most often proposed to make semantics depend. Dependence on psychology makes semantics full of vicissitudes; dependence on philosophy splits it into perennially surviving camps. It should be autonomous from both for the same reason that the Civil Service should be autonomous from elected parties.

Proposals to reduce semantics to something else have vastly exceeded proposals to make it autonomous. Bloomfield (1933 and earlier papers) is among the latter few. He states his view as an approval of Delbrück; but he and not Delbrück deserves the major credit because Delbrück's position would scarcely be known without Bloomfield and because he shows more elaborately how to carry out the program. Hjelmslev too has stressed the importance of autonomy.

As history proves, it is hard to keep the two stresses in balance. Few thinkers have seen that the *twofold* relation is needed. And neither of the two relations can be achieved by an automatic method. Autonomy admits of degrees; and there is autonomy by abstraction and autonomy by fiction. Autonomy by abstraction simply ignores those facts from another domain whose description would require alien concepts and propositions. Autonomy by fiction, on the other hand, is used where facts belonging to another domain are used and are described in a manner convenient to the borrowing science, but without commitment to the belief that the science in whose domain these facts belong will accept that manner of description. (For instance, a semantical account of the temporal process of interpretation may break up this process into stages; but these stages will be fictive in that admittedly the science of psychology, to whom the proper description of this process belongs, may not recognize these same stages as psychological realities.)

There is a paradox in Bloomfield's actual position which recent criticism has pointed up. (See Schlauch 1946, Basilius 1952, Olmsted 1952, Fries 1954.) Bloomfield tells linguists to steer clear of psychology by being mechanists. But there are three points we must make in reply. (i) The advice to steer clear of psychology, i.e. to autonomize, is good advice even if the proposed way of doing it is poor and is inconsistent with the advice itself. (ii) In addition to overstressing autonomy at the expense of reducibility (see above), Bloomfield is only acquainted with autonomy by abstraction and not with autonomy by fiction. Thus he cuts off linguistics (including semantics) from one of the means of following his own advice. (iii) In diagnosing the conflict between mentalism and mechanism he mislocates the issue and thus plants his germ of truth in the sand of confusion.

The second and third points need to be elaborated, because it is there that Bloomfield fails where Wittgenstein succeeds.

The day of scientific semantics is far off—this is the tenor of Bloomfield (1933) —for two reasons. First, semantics when it *is* reduced to psychology must be reduced to a mechanistic, not to a mentalistic psychology. Second, Bloomfield evidently has the conception that this mechanistic psychology will itself be reduced to physiology, including neurology. (It is this conception of psychology as reduced that identifies him with the era of Watson and Weiss rather than with that of Hull.) Autonomy of linguistics from psychology, and of psychology from physiology, is something to be put up with, endured; it is a stopgap, a makeshift. (The semantical techniques available at the present time are, for the most part, "makeshift devices"—(1933) p. 140.) In this frame of mind one can have no very great enthusiasm for devising such makeshifts.

To set things to rights we need a better understanding of the mentalism–mechanism issue. In rejecting mentalism Bloomfield has only one argument: that mentalistic explanations are tautologous, circular, empty And he goes on to identify, in effect, three contrasts: mentalistic versus mechanistic, philosophical versus scientific, and empty versus significant. But he is doubly mistaken: not all mentalistic statements are empty, and not all empty statements are mentalistic.

We may make the idea of emptiness more precise by following up Bloomfield's remark that an explanatory statement of the form 'B because A' is empty when B is the only evidence for A. E.g. it would be empty to say 'He ran because he was frightened' if the only evidence for his being frightened was that he ran. For in that case the statement that he was frightened 'adds nothing' to the statement that he ran.

Presumably this is offered as a necessary as well as a sufficient condition for emptiness; i.e. an explanatory statement is empty under this condition and not otherwise. Thus we can expand the remark in the preceding paragraph to read: An explanatory statement of the form 'B because A' is empty when and only when B is the only evidence for A. This remark and the phrase about 'adding nothing' are the clues we have to follow up.

The barest tautology would be of this form: 'B because B'. If on the other hand we say 'B because B and A' or 'B and A, therefore B', where A is something not implied by B, then we have added something. 'B because B and A' is no longer a tautology, because B is not the only evidence for B and A. A is also evidence for it, so that we are equally justified in asserting, 'A because B and A'.

This will be sufficient to establish our present point: that tautologies in Bloomfield's sense are seldom met with—or never. 'He ran because he was frightened' is not such a tautology, for there *is* other evidence for his fright in addition to his running. His running may be the most obvious and most near-at-hand evidence, and it may even be the only actual evidence known to us; but it is not the only possible evidence, i.e. not the only fact that would count as evidence. What I am saying here is only a commonplace of current philosophy of science.

Sometimes language deceives us. Consider for example the explanation, 'Wolves are fierce because it is their nature to be fierce'. Someone with a tough mind might inform us that this really means, 'Wolves are fierce because wolves

are fierce'. But even if so, the latter statement is not a tautology; translation into a symbolic language would make us realize that it is best taken as meaning 'This wolf is fierce because all wolves are fierce, and that wolf is fierce because all wolves are fierce, and the other wolf is fierce because all wolves are fierce, etc.'.

There are further problems involved, which we need not go into, such as the relation of 'description' to 'explanation'. Bloomfield would probably say that even though he had inadequately characterized 'tautology', he would still want to rule out as idle statements like 'This wolf is fierce because all wolves are fierce'. Now as it happens, what he seems to want has been shown by recent philosophy of science to be hopeless. What he wants, in principle, is to dispense with all 'constructs' and 'intervening variables', and to correlate observables directly with observables.

Seeing that the hopelessness of this widespread dream was not shown until the mid thirties, and is still not common knowledge (see Stevenson, 1944, pp. 46–53 for a good discussion), we need not blame Bloomfield for ignorance. It is enough to recognize that what was plausible in his day is not now plausible two decades later.

The upshot of the last few paragraphs is the following. We cannot throw out mentalistic explanations on the ground of tautology, for tautology in what appears to be Bloomfield's sense is neither essential to them nor peculiar to them. In the second place, we cannot throw them out on the ground of using 'constructs' either, because the present day view is that practically all science needs to make use of constructs. I conclude that whatever Bloomfield's valid point may have been, we cannot accept his formulation of it.

Actually, it is not difficult (with the help of present-day insights) to state Bloomfield's point more aptly. 'Avoid tautological explanations; be sure to state your explanations fully and precisely enough so that others can tell whether they are tautological or not; and avoid making use of "dispositions to respond" and other constructs *whenever possible*.' This is still good advice.

As remarked some paragraphs earlier, Bloomfield identifies three contrasts: (1) mentalistic vs. mechanistic, (2) empty vs. significant, and (3) philosophical vs. scientific. The independence of the first two has now been shown. The third is also independent of the first two. The reasons for holding this view are deliberately excluded from the scope of the present paper; but it may be remarked in passing that it is to Wittgenstein that the world is indebted for them.

4. Mill, Bloomfield, and circumstances. John Stuart Mill's 'methods of experimental inquiry' are well-known—too well known, indeed, since the time used for their teaching might better be spent on more up-to-date techniques. Mill states his 'Method of Agreement' as follows (*Logic* III. viii.1): "If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree, is the cause (or effect) of the given phenomenon."

Now the concept of stimulus and response is simply this conception of cause and effect, applied to psychology. And Bloomfield undertakes to reduce the

semantical concept of meaning to the psychological concept of stimulus and response. Knowing these two facts, we can *predict* that he will define meaning in more or less the following way: the meaning of a linguistic form is "the situation in which the speaker utters it and the response which it calls forth in the hearer" (1933, p. 139; cf. 1926, § 6; Bloch and Trager 1942 Section 1.2(2); Harris 1940 p. 227). I lay stress on the point that Bloomfield's definition, not in detail but in general, is a simple consequence (whether consciously or unwittingly so does not matter) of applying to this particular subject matter the general conception of cause formulated by Mill; for we may then expect that any difficulties in Mill's conception will be matched by difficulties in Bloomfield's. Now in Mill the principal difficulties concern (i) the plurality or unicity of causes and (ii) the notion of 'circumstance'.

(i) Mill speaks of 'the circumstance in which alone all the instances agree'. But is there any guarantee that there is such a circumstance? What if we find a number of instances of a phenomenon without *any* obvious circumstance in common? In that case we invent or postulate some non-phenomenal circumstance, thus preserving by postulation the unicity of causes. Analogously, Bloomfield speaks (1933, p. 141) of "the . . . features . . . which are common to all the situations that call forth the utterance of the linguistic form"; and to guarantee that there are such features we may have to postulate unobservables.

The problem becomes acute in connection with displaced speech. It is Bloomfield's merit to have played up the difficulties of this phenomenon, and a proof of his honesty that he acknowledges the difficulties to be greatest for his own kind of explanation. The discussion in (1933), pp. 141–4, suggests that he thinks the answer will be found in "obscure internal stimuli" (143). In addition to the prying question whether 'obscure' means 'hard to observe' or 'impossible to observe (unobservable)', there is a deeper objection. If we are entitled to invoke obscure internal stimuli to handle displaced speech, why not handle *all* speech by this means? Why not say that the feature common to all utterances of 'Oh, look! There's an apple' is one of these obscure internal stimuli, which, therefore, by Bloomfield's definition, is the meaning of that sequence of sentences? And this question shows us that Bloomfield's definition has simply failed to capture the ordinary meaning of 'meaning'.

(ii) There is another side of the coin. If we refuse to posit non-phenomenal causes, we may not find common 'circumstances' or 'features'. On the other hand, if we admit just any complex of phenomena, we will have too many common circumstances rather than too few. If we accept Russellian logic, we can prove a priori that for any class of phenomena there is a property (circumstance, feature) common to all of them and peculiar to them. And in any case it is usually not hard, given such a class, to find some feature—not at all far-fetched—some feature of spatial position, or the fact of their all occurring within a certain time period, etc., that is common to them all. So if Mill's methods are ever to be applicable, 'circumstance' must mean '*relevant* circumstance'.

This now standard criticism has its counterpart for Bloomfield. If we merely define the meaning of an utterance as the feature common to all (and only?)

the occasions on which it is uttered, we may find, instead of the one feature we want, either none or several—too few or too many. Some further limitation must be imposed. If a commonsense notion of what makes a feature is to be used, at least that notion should be examined. In applying the notion of a relevant feature we should not flop from the demands of an impossibly rigorous theory to the flaccid sanctions of common sense.

It is at this point that Wittgenstein comes to our aid.

5. Russell. Nearly fifty years ago Russell published "that paradigm of philosophy" (Ramsey, 1931, p. 263), the theory of descriptions. By a 'description' or more precisely a 'singular description' or 'definite description' he meant an expression of the form 'the so-and-so', provided it be used to refer to one and only one thing (as in 'the author of *Waverley*') and not to a kind (as in 'the elephant is a pachyderm'). His analysis led him to four closely related ideas:

(1) Every proposition has a logical form, and it may also have constituents arranged in this form. There can be two propositions with the same form but different constituents; also, two propositions with the same constituents but different forms.

(2) There may be a discrepancy between the apparent logical form of a proposition and its real logical form. In that case, the apparent logical form is created by a sentence which faultily expresses that proposition.

(3) In particular, propositions containing singular descriptions appear to have the same logical form as propositions containing genuine proper names (e.g. 'The wall I am looking at is yellow' seems to have the same logical form as 'That is yellow'). Furthermore, those expressions that are commonly taken to be proper names do not satisfy the logician's definition of 'proper name', but they function rather as substitutes for descriptions. (E.g. 'Homer' functions as a substitute for 'the man who was called 'Homer'. ') Finally, to complete the paradox, the words that do satisfy the logician's definition of 'proper name' are words such as 'I', 'this', 'here', 'now', which are traditionally classed as pronouns and adverbs.

(4) By a technique called 'definition in use' or 'contextual definition' it is possible to replace any sentence containing a singular description by a different sentence, expressing the same proposition, but which expresses that same proposition aptly, because its apparent logical form coincides with the real logical form of the proposition it is expressing.

Thesis (4) is put forward in Russell (1905); (3) in the same place and more elaborately in Russell (1911); (1) in Whitehead and Russell (1910) and more elaborately later; (2) is the latest to crystallize—there are hints in Whitehead and Russell (1910) pp. 2 (3) and 31 middle—, but it is paraphrased by Wittgenstein (1922) 4.0031: "Russell's merit is to have shown that the apparent logical form of the proposition [translate: 'sentence'—RW] need not be its real form." Then it is fully worked out, with examples, by Stebbing (1930, pp. 35, 79, 126ff., 139), Ryle (1932), and Kneale (1936). Flew (1951, p. 7) calls this thesis (2) that "which is most central and most fundamental to . . . the dominant tendency in modern British philosophizing".

The technique of thesis (4) is of vast though unexploited significance for scientific semantics; but we shall not take it up in the present paper. (1) and (3) will also have to be set aside. It is (2) and the developments from it that concern us.

The contrast of apparent and real logical form immediately suggests two programs, one analytic and one constructive. On the analytic side, let us consider every expression in our ordinary natural languages, and, in every case where apparent and real logical form are not the same, replace the expression by another expression, synonymous with it, whose apparent and real forms *are* the same. (If the synonymous expression is a paraphrase of the original, and therefore belongs to the same language, all the better.) On the constructive side, let us try to build an ideal language in which there is never any discrepancy between apparent and real logical form.

Russell himself and the logical positivists went ahead with both programs, attaching particular importance to the second. Ordinary language because of its vagueness, ambiguity, and extensive use of ellipsis will often fail to provide a perfect paraphrase for a faulty expression, and thus we must have recourse to an artificial language in any case. The Wittgensteinians, on the other hand (the third of the three branches mentioned in Section 1), accepted the first program but rejected the second. They did so because of a new insight that Russell had overlooked.

Consider the two statements, 'Tame tigers growl' and 'Tame tigers exist'. These two expressions look alike, in that their grammar is the same: A plural noun phrase is followed by a verb. But semantically they are vastly different. The second can be paraphrased 'There are tame tigers', but the first cannot be paraphrased in any similar way. It makes sense to say that tame tigers growl, eat, etc., but not that tame tigers exist, eat, etc. When one falls into the language-trap of assimilating 'exist' to 'growl', the result is the Ontological Argument for the existence of God. In breaking away from this trap, the first reaction is to say as Kant did that existence is not a predicate (unlike growling). But this is only a first step, for it still speaks as though there were something called 'existence'. It is more straightforward to treat the matter quite linguistically and say that the *word* 'exists' simply does not work like the word 'growls'.

Russell paraphrases 'Tame tigers growl' first as 'All tame tigers growl' and then, more analytically, as 'All things that are tame tigers growl'. 'Tame tigers exist' he paraphrases as 'There are tame tigers'. So far the Wittgensteinians would agree with him. But in going further and holding that the final paraphrases more accurately represent the real logical form, he makes a metaphysical commitment which they regard as unnecessary. For purposes of explaining how language works, there is no need to decide which of various synonymous expressions captures the real logical form; not even any need to suppose that there is a real form to be captured. It is sufficient to point out two things: (a) that an expression A can sometimes be paraphrased by an expression B that is of an apparently very different grammar; and (b) that there can be another expression C, apparently similar in grammar to A, but not admitting of the same kind of

paraphrase. In both cases, we do not compare the expression with reality but with other expressions.

This retreat from metaphysics marks the transition from Russell to Wittgenstein.

6. Wittgenstein. Wittgenstein has avoided slogans and generalities, but Wisdom (1953, p. 117) has offered this epitome: 'Don't look for the meaning, look for the use.'

We can see two of Russell's ideas converging and developing into this formula. One is the idea that singular descriptions have no meaning but do have use, because they *contribute* to meaning (Russell 1938, p. x). The other is that we exhibit the use of an expression by seeing how it can be paraphrased, what kinds of reasoning it can be used in, under what circumstances it is used, and so on. The formula does not mean that no expression has meaning; it means that meaning is a special kind of use, and that every expression has use even if it has no meaning. This will be illustrated by examples. The formula tells us not to force all uses into the mold of meaning.

Now we begin to make contact with the historical remarks in Section 2. Wittgenstein is offering a way out of the dyadic conception of meaning. His way will be interesting if, and only if, he can tell us or teach us *how* to determine uses.

Probably the best expository method at this point is to give a number of examples and then see what will emerge from an inductive study of them.

I will select for this purpose a number of English adjectives, and discuss the different 'uses' that adjectives may have and the techniques by which these differences are brought to light. I assemble under this heading a number of examples that have been separately discussed by various Russellians and Wittgensteinians. In general, the examples that are simple and also scientifically interesting are not the ones of greatest importance for philosophy, so this selection cannot give any inkling of why the Wittgensteinian approach should be creating such excitement among philosophers.

Let us consider the following groups of adjectives: (1) All, some, no; (2) One, ten; (3) Big, little; (4) Numerous; (5) Grey, sweet, loud; (6) Former, future; (7) Apparent, real; (8) Spoiled, addled.

We may as well begin with a classic passage from *Through the Looking-Glass:*

> "Who did you pass on the road?" the King went on, holding out his hand to the messenger for some hay.
>
> "Nobody," said the messenger.
>
> "Quite right," said the King: "this young lady saw him too. So of course Nobody walks slower than you."
>
> "I do my best," the messenger said in a sullen tone. "I'm sure nobody walks much faster than I do."
>
> "He can't do that," said the King, "or else he'd have been here first."

Flew (1951, p. 8) comments thus: "When they ["philosophers"] have talked of misunderstanding the logic of our language [cf. Wittgenstein, 1922, 4.003] they have been alluding to the mistakes made by people who are misled by grammatical similarities or dissimilarities into overlooking logical dissimilarities or

similarities. Such is the mistake of the King who treats 'Nobody' as if it had the logic of 'Somebody', as if 'Nobody' referred to somebody, albeit a rather insubstantial somebody." Actually, it would be more accurate to describe the mistake as supposing that the word 'nobody' is an ordinary proper name, like 'Alice'. For even if in the above passage we replaced 'nobody' by 'somebody', there would still be the same fallacy: the conjunction of 'Somebody walks slower than you' and 'Somebody walks much faster than you' does not entail 'Somebody walks both slower and much faster than you', although the conjunction of 'Alice walks slower than you' and 'Alice walks much faster than you' does entail 'Alice walks both slower and much faster than you'. The Wittgensteinians refer to this as a difference between the 'logic' of 'somebody' and of 'nobody', on the one hand, and of 'Alice' on the other. Or as a difference between the way the former two expressions work, and the way the latter works. Or as a difference in 'grammar'.

It is important to notice that both the word 'logic' and the word 'grammar' are used by the Wittgensteinians in a broader sense than is usual. It is natural to call the above-discussed difference a difference in logic, but not to call it a difference in grammar. Elsewhere, e.g. in group (5), we shall come across differences that it would be natural to call differences in grammar, but not in logic. Thus we have to inquire why, in the hands of Wittgenstein, the meanings of the two words have converged.

One difference of 'logic' or 'grammar', and an obvious one, between 'some' and 'no', is that from 'He saw Alice' one may infer 'He saw somebody', but not 'He saw nobody'. On the other hand there are features of logic or grammar that they share, e.g. that from 'He saw somebody' one may infer 'Somebody was seen by him', and that similarly from 'He saw nobody' one may infer 'Nobody was seen by him'.

The difference between 'all' and 'every' seems quite trivial, and merely grammatical in the narrowest sense. When a noun has both a singular and a plural, 'all' takes the plural and 'every' takes the singular—without further difference in meaning. 'All persons are here', 'Every person is here', 'Everybody is here' have the same meaning—are interchangeable.

But compare the difference between 'every' and 'all' with the difference between 'one' and 'ten'. 'One', like 'every', takes the singular, and 'ten', like 'all', takes the plural. But the parallel ends there. We could cut short discussion by the dictum that 'one' just doesn't mean the same thing as 'ten', or we could give evidence for our claim by such consideration as the following:

(A) Every person is here; therefore, all persons are here
(B) One person is here; therefore, ten persons are here

(A) would be accepted as a valid inference, but (B) would not.

'One' and 'ten' raise a difficult problem. We have a deep-seated feeling that they belong together, but it is not easy to justify it in detail. There is no trouble about finding features that they share; e.g. one can say—truly or falsely, but at least not nonsensically—'One and one make twenty', 'Ten and ten make twenty',

whereas it would be nonsensical to say 'Some and some make twenty' or 'Big and big make twenty'. There is no trouble about finding features that unite 'one' and 'ten' and that separate them both from 'some' and from 'big'; the problem is to show that these features are fundamental. This is something that we have to pass over in the present paper.

'Big' and 'little' seem like typical adjectives; so do 'grey', 'sweet', 'loud'. Yet there is an important difference between 'little' and 'grey', for example. It is a difference of logic, rather than of grammar, in the ordinary sense of these words. Namely, we can argue: 'An elephant is an animal; therefore a grey elephant is a grey animal', but we cannot similarly argue: 'An elephant is an animal; therefore a little elephant is a little animal'. The linguist must face the problem, *Just how* shall this difference be described?, but in any case there it is.

Wittgenstein would call this difference a difference in logic, in grammar, or in logical grammar. Thus, 'grammar' in his sense encompasses meaning and semantics in the ordinary sense.

The adjective 'big' and 'little' enable us to make a simple point about the relationship between meaning, use, and grammar. These two words (and pairs of antonyms in general) would be said to have different meanings but the same grammar (or the same logic). Presumably they would be said to have different uses.

'Grey', 'sweet', and 'loud' raise two other problems. A cloud would be called grey but not sweet or loud, a grapefruit sweet or sweet-tasting but not grey-tasting nor loud, a noise loud and sweet but not grey. To this extent 'grey', 'sweet', and 'loud' have different logics. (i) But we find ourselves inclined to say that a sweet noise is not sweet *in the same sense* as a sweet grapefruit; what shall we say about this inclination? (ii) If we distinguish two senses of 'sweet', then the three terms 'grey', 'sweet' (as applied to flavors), and 'loud' are mutually exclusive: there is nothing to which two of them apply. Now although this difference in logic would be granted, we all feel that it is comparatively minor, in contrast to the 'logical' features that they have in common. I refer to the fact that they are all names of sense-qualities. The problem, then, is this: On what grounds can we justify the classification of some resemblances or differences of logic as major, and others as minor?

The logic of 'numerous' includes some features that it shares with 'all', 'ten', and 'big'. To put it in terms that a linguist would use, there are linguistic environments in which all of these words can occur. Of the peculiarities of the word, some are such as familiar linguistic means would bring to light—e.g. that none of the other three words can replace 'numerous' in 'Enemies of the people are growing more numerous by the minute'; others require techniques of inference such as the following: From 'Enemies of the people are big' one can infer 'Some enemies of the people are big', but one cannot infer 'Some enemies of the people are numerous' from 'Enemies of the people are numerous'. (This is one aspect of the traditional distinction in logic between distributive and collective terms.)

So far we have two kinds of facts about 'numerous': (1) Grammatical facts in the formal linguistic sense. (2) Facts of inference. In addition there are two

other kinds of facts: (3) Facts of paraphrase or synonymy: our sample sentence is synonymous with 'The number of enemies of the people is increasing by the minute.' (4) Facts of circumstances of use.

The fourth class is the one about which we have said the least. It is here that semantics in the ordinary sense is taken into account. *When* do we use the word 'numerous'?

It is easy and uninteresting to answer that we say the so-and-so's are numerous when we believe that there are a lot of so-and-so's. It is equally easy, and still more uninteresting, to answer that we say it when our neural system is in a certain state. Between these two extremes the approach of Wittgenstein gives us a sensible, helpful answer. (See end of Section 4.)

First, we may point out the difference between uttering and asserting. We assert 'Enemies of the people are numerous' when we make this a self-contained utterance; we utter it without asserting it when we say 'If enemies of the people are numerous, then we are in peril', or 'It isn't true that enemies of the people are numerous' or 'He says that enemies of the people are numerous', etc. To classify the circumstances under which we can utter something without asserting it is one of the numerous jobs of detail that remains to be done.

Second, we may note the different ways in which something can be asserted: as a lie, as a joke, as an exaggeration, as a sober truth, etc. Another job of classification. (Wittgensteinians have spent a great deal of effort on this job, but without much success so far.)

Third, we may look into the circumstances under which people assert, as a sober truth, that enemies of the people are numerous. Perhaps the most striking facts are that the word 'numerous' is used (1) relatively and (2) vaguely. (1) In a country of a million people ten enemies would not be numerous, whereas in a page of 300 words ten misprints would be more than enough to be numerous. This relativity is like that of 'big' and 'little': there must be some class of things to which implicit or explicit reference must be made. (2) Once the reference class is established, there is still vagueness. In a country of a million people, ten enemies would not be numerous and 100,000 enemies would be numerous, but where is the dividing line? There is none; there is no 'line'; instead, there is a border 'area', itself vaguely delimited, about which people are uncertain. The linguistic display of uncertainty when dealing with this area is instructive. Sometimes people hedge: Suppose that 5,000 is within this area: 'So there are 5,000 enemies of the people in our country? Well, then, they're not so very numerous. And yet when you think about it that *is* quite a lot, isn't it?' Or they qualify: '5,000 enemies? That's relatively numerous.' Or, and most interesting of all, they simply withdraw the word: 'Are enemies of the people numerous in our country? Well, there are about 5,000 of them.' This fact of withdrawal is one of the most distinctive marks of ordinary language, as contrasted with the artificial languages of the symbolic logician: the fact of including expressions which are clearly applicable in some cases, clearly inapplicable in others, simply not used in others; and in still other cases, used but withdrawn if challenged, and replaced by something more precise.

'Former' and 'future'. 'The former mayor' looks like 'the big mayor', but whereas a big mayor is a mayor, a former mayor is not a mayor. Linguistically we describe 'former' as a back-formation: 'He is the former mayor' can be paraphrased, 'He was formerly the mayor'. 'Prince Charles is the future king' means the same as 'Prince Charles will be the king'. This is an obvious remark.

'Apparent' and 'real'. These words are back-formations just like 'former' and 'future', but the fact has been pretty universally overlooked. As a result, we often speak as though there were two classes of everything—the apparent ones and the real ones. It does not concern us here to trace the consequences for metaphysics, but we may remark on the complication caused by the fact that 'real' in another sense contrasts with 'spurious' or 'fake' and is synonymous with 'genuine', and in yet a third sense means 'par excellence' as in 'he's a real patriot'.

'Spoiled', 'addled'. "In glossing a word," Quine writes (1953, p. 58), "we have so frequently to content ourselves with a lame partial synonym plus stage directions. Thus in glossing 'addled' we say 'spoiled' and add 'said of an egg'." Stevenson (1944, p. 82) has distinguished these two techniques of glossing as defining versus characterizing—a distinction that plays a great part in his theory of emotive meaning.

We have now considered a miscellaneous sample of English adjectives and seem some of the semantically useful things that can be said about them. The techniques illustrated need to be systematized and, very likely, supplemented. Much of the help that the Wittgensteinians give can be found elsewhere, even in traditional writers. And it may be that much of what is new in Wittgensteinianism is mistaken. For instance, its broadened use of the terms 'logic' and 'grammar' in such a way as to make logic and grammar coincide has yet to be justified. Still and all, its impact will be refreshing. In my judgement its biggest contribution to linguistics is the help it gives us in treating meaning practically and autonomously, and at the same time in a way that does not preclude the hope of eventual reduction.

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PROBLÈMES SÉMANTIQUES DE LA RECONSTRUCTION

E. Benveniste

Les notions sémantiques se présentent encore sous une forme si vague que pour en traiter un aspect, il faudrait commencer par poser un corps de définitions rigoureuses. Mais ces définitions demanderaient à leur tour une discussion portant sur les principes mêmes de la signification. C'est une tâche longue et ardue, dont les travaux consacrés jusqu'ici à la sémantique ne donnent qu'une faible idée. Aussi, dans cette contribution qui se limite au thème suggéré par les éditeurs du présent recueil, nous aurons à procéder plutôt empiriquement, négligeant pour l'instant les considérations théoriques pour traiter dans le concret quelques types de problèmes que le linguiste rencontre quand il s'occupe de reconstruire.

En général les critères d'une reconstruction formelle peuvent être stricts, parce qu'ils découlent de règles précises, dont on ne peut s'écarter que si l'on se croit en mesure d'y substituer des règles plus exactes. Tout l'appareil de la phonétique et de la morphologie intervient pour soutenir ou réfuter ces tentatives. Mais en matière de sens, on n'a pour guide qu'une certaine vraisemblance, fondée sur le «bon sens», sur l'appréciation personnelle du linguiste, sur les parallèles qu'il peut citer. Le problème est toujours, à tous les niveaux de l'analyse, à l'intérieur d'une même langue ou aux différentes étapes d'une reconstruction comparative, de déterminer si et comment deux morphèmes formellement identiques ou comparables peuvent être identifiés par leur sens.

Le seul principe dont nous ferons usage dans les considérations qui suivent, en le prenant pour accordé, est que le «sens» d'une forme linguistique se définit par la totalité de ses emplois, par leur distribution et par les types de liaisons qui en résultent. En présence de morphèmes identiques pourvus de sens différents, on doit se demander s'il existe un emploi où ces deux sens recouvrent leur unité. La réponse n'est jamais donnée d'avance. Elle ne peut être fournie que par une étude attentive de l'ensemble des contextes où la forme est susceptible d'apparaître. On n'a pas le droit de la présumer, positive ou négative, au nom de la vraisemblance.

§1. Soit par exemple le cas des homophones anglais *story* 'narrative' et *story* 'set of rooms'. Ce qui fait obstacle à leur identification n'est pas notre sentiment qu'un 'récit' et un 'étage' sont inconciliables, mais l'impossibilité de trouver un emploi tel qu'un sens y soit commutable avec l'autre. Même des expressions choisies à dessein comme ambiguës telles que *to build a story* ou *the third story* (d'un recueil—d'un immeuble), une fois replacées dans un contexte authentique, perdent immédiatement leur ambiguité. Il faut donc les tenir pour distincts. Et c'est seulement à titre de confirmation que la preuve étymologique sera utilisée: *story* 'narrative' < a.fr. *estoire* (*historia*), mais *story* 'floor' < a.fr. *estorée* (**staurata*). L'étymologie pourrait nous manquer; même donnée, elle ne suffirait pas seule à garantir l'indépendance actuelle des deux morphèmes, qui auraient pu, en vertu de leur identité formelle, s'associer par leur sens en quelque manière et créer une unité sémantique nouvelle.

§2. Voici le cas inverse. Il y a en français *voler* 'fly' et *voler* 'steal'. Les deux verbes sont distincts à tous égards. L'un, *voler* 'fly' fait partie de la classe sémantique de 'marcher, courir, nager, ramper, etc.'; l'autre, *voler* 'steal' entre en synonymie avec 'dérober, soustraire, etc.' *Voler* 'fly' est intransitif; *voler* 'steal' est transitif. La dérivation ne comporte qu'un terme commun aux deux: *vol*. Autrement ils diffèrent: *voler* 'fly' entraîne *voleter*, *s'envoler*, *survoler*, *volée*, *volatile*, *volaille*, *volière*; mais *voler* 'steal' seulement *voleur*. Cette limitation même de *voler* 'steal' fait soupçonner qu'il se ramène à un emploi spécialisé de *voler* 'fly'. La condition en serait un contexte où *voler* 'fly' se prêterait à une construction transitive. On trouve ce contexte dans la langue de la fauconnerie; c'est l'expression «le faucon *vole* la perdrix» (= atteint et saisit au vol). Telle est la condition de fait, non présumable par avance, où l'emploi exceptionnellement transitif crée un nouveau sens de *voler*; dans cette situation le *vol* de l'oiseau signifie à la fois 'flying' et 'stealing'. La coexistence de deux *voler* ne doit donc pas provoquer à les concilier dans une unité improbable; la situation particulière de l'un des deux homonymes et notamment la pauvreté de sa dérivation incitent à rechercher l'emploi typique qui a introduit une scission dans un champ sémantique unitaire pour en faire deux domaines aujourd'hui distincts.

§3. Dans l'appréciation des différences de sens qui interviennent entre les membres d'un ensemble formellement lié, le linguiste est toujours enclin à se guider inconsciemment sur les catégories de sa propre langue. De là des problèmes sémantiques qui se ramènent, tout bien considéré, à des problèmes de traduction. On en rencontre même dans celles des restitutions qui n'ont jamais été mises en question et pourraient passer pour évidentes. La correspondance entre gr. *títhēmi*, *éthēka* 'poser' et lat. *facere* 'faire' est une donnée élémentaire de l'enseignement comparatif. D'où l'on conclut que **dhē-* admet à la fois le sens de 'poser' et celui de 'faire'. Mais entre 'poser' et 'faire' la connexion n'est pas pour nous si manifeste qu'on doive l'admettre sans justification pour l'indo-européen. Dans notre classification des notions, 'poser' se range avec 'placer, mettre, loger', etc.; et 'faire' avec 'accomplir, construire, fabriquer, opérer, etc.' Les deux lignes ne se rencontrent pas. La multiplicité même des acceptions de 'faire' ne semble pas contribuer à préciser la liaison pourtant impliquée dans ces rapprochements anciens. Pour fonder ce rapport de sens, on a allégué des emplois techniques.[1] En fait les raisons doivent être cherchées dans une définition plus précise des emplois. On doit observer d'abord que là même où la traduction 'poser' est admissible, les conditions de l'emploi montrent que 'poser' signifie proprement 'poser quelque chose qui subsistera désormais, qui est destiné à durer': en grec, avec *themetlia* 'poser les fondements', avec *bômon* 'fonder un autel'. C'est pourquoi il est apte à signifier 'établir dans l'existence, créer'; cf. en vieux-perse *būmīm adā . . . asmānam adā* 'il a posé (= créé) la terre, il a posé (= créé) le ciel', en grec *khármat' éthēken* 'il a posé (= créé) des joies pour les hommes' (Pind. *Ol.* 2.101), etc. En second lieu, on remarquera qu'une des constructions les plus fréquentes de **dhē-* est prédicative, ce qui fournit justement la condition du sens usuel de 'faire', aussi bien dans les langues qui

[1] Cf. Ernout-Meillet, *Dict. étym.*$_3$ 372 fin.

connaissent encore 'poser' que dans celles qui, comme le latin, ont seulement 'faire': *basiléa tina theînai*, c'est littéralement *aliquem regem facere;* une expression telle que *theînaí tina athánaton* équivaut exactement à *immortalem facere.* Il suffit d'indiquer le principe; les exemples abondent. L'important est de voir que: (1) la distinction de 'poser' et 'faire' ne répond pas à la réalité indo-européenne sous la forme tranchée qu'elle a pour nous; (2) la construction de **dhē-* est une composante essentielle de l'emploi et du sens; (3) la notion de 'faire', en tant qu'elle est exprimée par **dhē-*, se détermine par des liaisons particulières qui seules permettent de la définir, car la définition n'est possible que dans les termes de la langue même.

§4. Cette situation se présente souvent, sous des aspects parfois moins reconnaissables. On rencontre alors des difficultés qui peuvent tenir à ce que l'un ou l'autre des sens considérés est inexactement ou trop sommairement défini. Nous en prendrons un exemple dans le cas d'un verbe grec dont les sens n'ont semblé jusqu'ici créer aucun problème. Nous avons en grec *tréphō* 'nourrir', avec de nombreux dérivés et composés attestant le même sens: *trophós* 'nourricier', *tropheús* 'nourrisseur', *trophḗ* 'nourriture', *dio-trephḗs* 'nourrisson de Zeus', etc. Il est déclaré identique à *tréphō* 'épaissir, coaguler (un liquide)', pf. *tétrophe* 'se coaguler, être compact', qui à son tour a été rattaché à *thrómbos* 'caillot de sang' (malgré la phonétique), puis à une série de comparaisons incohérentes dont on trouvera le détail chez Boisacq 353 et qui ne nous retiendront pas ici. Seule nous importe la relation en grec même de *tréphō* 'nourrir' et de *tréphō* 'cailler (le lait)'. Il est fort possible en effet que les deux sens n'en fassent qu'un, mais comment se rejoignent-ils? Les dictionnaires ne marquent aucun embarras. Celui de Liddell-Scott-Jones définit ainsi *tréphō*: «1. *thicken* or *congeal* a liquid. 2. usu. *cause to grow* or *increase, bring up, rear*, esp. of children bred and brought up in a house». De même Bailly: «1. rendre compact. 2. rendre gras, engraisser, nourrir». Même à qui ne se fie qu'au «sentiment» de la langue, une pareille relation devrait apparaître si étrange qu'elle imposerait une vérification des emplois. Qu'on ait pu admettre comme évident que 'cailler (le lait)' conduit au sens de 'nourrir, élever (un enfant)' suffirait à discréditer cet empirisme «intuitif» qui sert de méthode dans la plus grande partie des reconstructions. Ici la disparité des sens semble telle qu'on ne pourrait les concilier que par un artifice. En réalité, la traduction de *tréphō* par 'nourrir', dans l'emploi qui est en effet le plus usuel, ne convient pas à tous les exemples et n'est elle-même qu'une acception d'un sens plus large et plus précis à la fois. Pour rendre compte de l'ensemble des liaisons sémantiques de *tréphō*, on doit le définir: 'favoriser (par des soins appropriés) le développement de ce qui est soumis à croissance'. Avec *paîdas, híppous*, on le traduira 'nourrir, élever (des enfants, des chevaux)'. Mais on a aussi *tréphein aloiphḗn* 'favoriser l'accroissement de la graisse' (Od. 13.410); *tréphein khaítēn* 'laisser croître sa chevelure' (Il. 23.142). C'est ici que s'insère un développement particulier et «technique», qui est justement le sens de 'cailler'. L'expression grecque est *tréphein gála* (Od. 9.246), qui doit maintenant s'interpréter à la lettre comme 'favoriser la croissance naturelle du lait, le laisser atteindre l'état où il tend', ou, prosaïquement, 'le laisser cailler'. Ce n'est rien autre qu'une

liaison idiomatique de *tréphein* au sens de 'laisser croître, favoriser la croissance' qu'il a partout. Au point de vue du grec, il n'y a pas de différence entre *tréphein khaítēn* 'laisser la chevelure se développer' et *tréphein gála* 'laisser le lait se développer'. Il n'y en a pas davantage entre *tróphies paîdes* 'enfants qui ont grandi (et atteint l'âge adulte)' et *kúmata trophóenta, kúma tróphi* 'vagues qui ont atteint leur plein développement'. Il n'y a donc plus de problème du classement des deux sens de *tréphō*, puisqu'il n'y a qu'un sens, partout le même. On peut conclure que *tréphō* 'cailler' n'existe pas; il existe un emploi de *tréphō gála*, qui crée une association pour nous insolite, mais explicable dans les contextes grecs. On voit aussi que toute la difficulté provient, au fond, des différences entre les ressources lexicales des langues considérées. Alors que *tréphein paîda* se rend directement en anglais ou en français ('rear a child, nourrir un enfant'), *tréphein gála* exige une traduction spécifique ('curdle milk, cailler du lait'). Le linguiste qui se demande: «comment concilier 'curdle' et 'rear', ou 'cailler' et 'nourrir'?» ou qui invente une filiation entre ces deux sens, est victime d'un faux problème. La question ne se pose ni dans une langue moderne, où les formes sont différentes, ni en grec, où les sens sont identiques. Ce n'est là qu'un exemple entre beaucoup des difficultés gratuites créées dans la reconstruction sémantique soit par une définition insuffisante des termes en discussion soit par une transposition illégitime des valeurs d'un système sémantique dans un autre.

§5. Le même problème pourra être posé, non plus à l'intérieur d'une langue historique, mais dans la synchronie d'une reconstruction formelle. Il y a en indo-européen une racine **dwei-* 'craindre' bien attestée par gr. *déos* 'crainte' (**dweyos*) et le parfait *dé-dwoi-a* 'j'ai crainte' fournissant le présent *deídō*, par av. *dvaēθā-* 'menace, motif de crainte', par le présent arm. *erknçim* 'je crains'. Ce **dwei-* 'craindre' est matériellement identique au thème du numéral **dwei-* 'deux'. La ressemblance persiste dans les dérivés de date historique: hom. *dé-dwoi-a* 'j'ai crainte' a l'air d'être bâti sur le même thème que l'adjectif *dwoi-ós* 'double', et arm. *erknçim* 'je crains' rappelle *erku* 'deux' (**dwō*); l'alternance dans le parfait homérique 1.sg. *dé-dwoi-a* : 1.pl. *dé-dwi-men* est conforme à celle du numéral **dwei-* (**dwoi-*) : **dwi-*. Bref, tout paraît indiquer une identité formelle entre ces deux radicaux. Est-ce un hasard? Mais pour exclure un hasard, il faudrait démontrer que l'identité formelle se vérifie dans le sens. Et quelle liaison de sens pourrait-on imaginer entre 'craindre' et 'deux' qui ne ressemble à un jeu d'esprit? Il faut néanmoins y regarder plus attentivement, et ne pas repousser sans examen la possibilité d'un rapport. Car—cela est essentiel—si nous pouvons considérer comme «simple» la notion de 'deux', nous n'avons aucun droit de présumer également «simple» une notion telle que 'craindre'. Rien ne nous assure a priori qu'elle ait eu la même structure sémantique dans des états anciens de l'indoeuropéen que dans la langue de nos propres raisonnements. Et l'analyse de cette structure sémantique a elle-même pour condition l'étude des emplois de **dwei-* «craindre» là où nous pouvons le mieux les observer. Le grec homérique se prête à une pareille étude, et il la récompense. Car c'est dans un texte de l'Iliade, mille fois lu et relu pourtant, que la solution s'offre, encore inédite. Voici le passage: *líēn méga pêma . . . eisoróōntes déidimen; en doiêi dè saōsémen è*

apolésthai nêas (Il. 9.229–230), littéralement: 'prévoyant un grand désastre, nous avons peur (*deídimen*); ce qui est en doute (*en doiêi*) est: sauverons-nous ou perdrons-nous les vaisseaux?' Le texte même, rapprochant dans la même phrase *deídimen* et *en doiêi*, éclaire, comme par une démonstration d'école, leur relation. L'expression *en d(w)oyêi*[2] (*esti*) signifie proprement 'la chose est en double, en doute, *in dubio*', c'est à dire 'elle est à *redouter*'. D'où il suit que **dwei-* 'craindre' signifie 'être en double, *douter*' au sens où *douter* est pris en ancien français (= fr. mod. *redouter*). La situation décrite dans le texte cité (sentiment devant une alternative périlleuse) restaure la liaison cherchée entre **dwei-* numéral et **dwei-* verbal. On peut désormais les identifier pour le sens. A titre subsidiaire, on utilisera des parallèles tels que lat. *duo*, *dubius* (*in dubio esse*), *dubitare*; all. *zwei*, *zweifeln*, etc. Ainsi, grâce à un contexte décisif, se configure en indo-européen une notion telle que 'craindre' avec ses liaisons spécifiques que seul l'emploi peut révéler, et qui sont différentes de celles qui la déterminent aujourd'hui.[3]

§6. La nécessité de recourir aux contextes pourrait sembler un principe de méthode trop évident pour mériter qu'on y insiste. Mais quand on ramène le sens aux variétés de l'emploi, il devient impératif de s'assurer que les emplois permettent non seulement de rapprocher des sens qui paraissent différents, mais de motiver leur différence. Dans une reconstruction d'un procès sémantique doivent aussi entrer les facteurs qui provoquent la naissance d'une nouvelle 'espèce' du sens. Faute de quoi la perspective est faussée par des appréciations imaginaires. Nous en prendrons un exemple dans un rapprochement banal entre tous, celui de lat. *testa* et de fr. *tête*. On va répétant que le passage du sens de *testa* 'cruche; tesson' à celui de *tête* serait dû à une dénomination de plaisanterie. L'explication se trouve jusque dans les plus récents dictionnaires.[4] Il serait temps de voir les faits, qui d'ailleurs sont clairs et qu'on a seulement omis de considérer. Le problème commence avec le nom de la 'tête' en latin classique. On constate que *caput* ne signifie pas seulement 'tête', mais aussi 'personne', et aussi 'capital (financier)' et aussi 'capitale (d'une province)'; il entre dans des liaisons telles que *caput amnis* 'source (ou embouchure) d'un fleuve', *caput coniurationis* 'chef de la conjuration', *caput cenae* 'pièce principale du repas', *caput libri* 'chapitre d'un livre', *caput est ut* . . . 'il est essentiel de . . .', etc. Le nombre et l'étendue de ces variantes affaiblissaient la spécificité de *caput* 'tête', ce qui conduisait à deux solutions possibles. Ou bien on le redéterminait comme **caput corporis*, qui aurait été lui-même ambigu et que, en tout cas, la langue a repoussé; ou bien on le remplaçait par un terme différent. C'est ce qui s'est produit en latin même, par recours à *testa*, qui désignait toute coquille dure, et qui s'est d'abord appliqué à ce que nous appelons encore la 'boîte crânienne' (cf. brain*pan*, Hirn*schale*). Le sens de 'crâne' apparaît clairement en latin tardif[5] (An-

[2] La forme du dat. gr. *doiêi* remonte à **dwoyyāi* et répond au dat. sg. f. skr. *dvayyái* (Wackernagel, *Nachr. Gött. Ges.* 1914.119).

[3] Cette démonstration était inédite. J'en avais cependant indiqué la conclusion par lettre à J. Pokorny qui en fait mention dans son *Idg. Etym. Wb.* (1949) 228.

[4] Cf. Bloch-Wartburg, *Dict. étym.*[2] (1950) 602.

[5] Les principaux exemples ont été réunis par E. Löfstedt, *Syntactica* 1 (1933) 352, avec la conclusion correcte qu'ils imposent. Mais personne ne semble en avoir tenu compte.

toninus Placentinus: *vidi testam de homine* 'j'ai vu un crâne d'homme') et il servait déjà à dénommer la 'tête': *testa:* caput vel vas fictile (CGL. V,526.39), d'où en ancien français *teste* 'crâne'. Il est probable que, comme terme anatomique, *testa* était en usage chez les médecins romains longtemps avant que les textes le mentionnent. Il n'y a donc dans ce procès ni plaisanterie, ni à vrai dire singularité propre à fixer l'attention. On pourra même trouver que le cas de *testa*: *tête* a usurpé la place qu'il tient dans l'enseignement traditionnel; il offre simplement un aspect particulier du renouvellement qui a atteint la plupart des noms de parties du corps. De là se dégagent les oppositions successives: lat. *caput: testa* > a.fr. *chef*: *teste* > fr. mod. *tête*: *crâne*. Mais dans cette perspective rectifiée, les considérations sur *testa* comme désignation humoristique ne paraissent plus fondées. La véritable question serait plutôt d'étudier comment coexistent et se délimitent respectivement *caput* et *testa* en latin tardif, *chef* et *teste* en ancien français, pour conduire à la répartition actuelle. Si cette recherche reste encore à faire, c'est en partie au moins parce qu'une appréciation inexacte de la nature du procès en a obscurci la portée.

§7. Dans le cadre d'une comparaison à grande échelle, mettant en œuvre plusieurs langues, on constate souvent que des formes évidemment apparentées se distinguent chacune par une variété particulière de sens. Quoique l'unité sémantique de la famille soit indéniable, elle ne semble pas pouvoir se définir exactement. On a l'impression que le 'sens premier', conservé exactement par une langue, s'est trouvé dévié par des raisons particulières dans chacune des autres, produisant ainsi une image composite de la situation sémantique. En général les comparatistes ne s'attardent pas à l'examiner, quand les correspondances formelles sont satisfaisantes. Ou s'ils considèrent le sort propre d'une des formes, c'est sans égard à la question d'ensemble. Tel est, par exemple, le cas du nom du 'chemin': skr. *pánthāḥ*, av. *pantå*, arm. *hun*, v.sl. *pǫtĭ*, v.pr. *pintis*, gr. *póntos*, lat. *pons*. L'antiquité indo-européenne du terme est garantie par les archaïsmes de la flexion. On ne saurait dire que le sens fasse obstacle à la restitution d'une forme commune. Néanmoins les divergences apparaissent assez sérieuses pour justifier une question. En indo-iranien, slave et baltique, il s'agit du 'chemin'. Mais gr. *póntos* signifie 'mer'; lat. *pons* désigne le 'pont', et arm. *hun*, le 'gué'. Comme ces sens ne s'équivalent pas et que, dans la distribution dialectale, c'est spécialement en grec et en latin que la divergence se manifeste, on tend à penser que ce désaccord tient à des raisons de style ou de culture. En grec c'est une figuration poétique qui aurait assimilé la 'mer' à un 'chemin'. En latin, le transfert de 'chemin' à 'pont' résulterait de la civilisation des terramare.... Ces hypothèses ont pour fondement une autre hypothèse, non reconnue comme telle, informulée et inconsciente: le sens premier serait celui de 'chemin', soit parce qu'il est attesté dans un dialecte ancien tel que l'indo-iranien, soit à cause de l'accord entre l'indo-iranien, le slave et le baltique, soit en vertu de sa «simplicité»; et les sens de 'mer' ou 'pont' ou 'gué' en seraient des déviations. Mais les emplois dont nous disposons dans les textes anciens les plus abondants, en védique,[6]

[6] Les principaux exemples védiques ont été utilement rassemblés par P. Thieme, *Der Fremdling im Rgveda* (Leipzig, 1938), 110–117.

permettent d'accéder à une notion plus exacte de *pánthāḥ*, et d'en nuancer la représentation. D'abord, il y a en védique plusieurs autres noms du chemin, et qui tous en quelque manière se distinguent de celui-ci: *yāna-* dénomme le 'chemin' des âmes vers leur séjour (*devayāna*, *pitṛyāna*); *mārga-*, le sentier des animaux sauvages (*mṛga*); *adhvan*, la route frayée; *rāthya*, la voie des chars. Ce qui caractérise le *pánthāḥ*, est qu'il n'est pas simplement le chemin en tant qu'espace à parcourir d'un point à un autre. Il implique peine, incertitude et danger, il a des détours imprévus, il peut varier avec celui qui le parcourt, et d'ailleurs il n'est pas seulement terrestre, les oiseaux ont le leur, les fleuves aussi. Le *pánthāḥ* n'est donc pas tracé à l'avance ni foulé régulièrement. C'est bien plutôt un 'franchissement' tenté à travers une région inconnue et souvent hostile, une voie ouverte par les dieux à la ruée des eaux, une traversée d'obstacles naturels, ou la route qu'inventent les oiseaux dans l'espace, somme toute un chemin dans une région interdite au parcours normal, un moyen de parcourir une étendue périlleuse ou accidentée. L'équivalent le plus approché sera plutôt 'franchissement' que 'chemin', et c'est bien ce sens qui explique les diversité des variantes attestés. A partir de skr. *pathya* et dans l'histoire de l'indo-aryen, nous avons 'chemin', mais ce sens n'est pas plus «originel» que les autres; ce n'est qu'une des réalisations de la signification générale ici définie. Ailleurs, ces réalisations sont représentées autrement. En grec, le 'franchissement' est celui d'un bras de mer (cf. *Hellés-pontos*), puis plus largement d'une étendue maritime servant de 'passage' entre deux continents; en arménien, d'un 'gué'; et en latin, *pons* désignera le 'franchissement' d'un cours d'eau ou d'une dépression, donc un 'pont'. Nous ne sommes pas en mesure de donner les raisons précises, qui tiennent à la géographie ou à la culture, de ces déterminations particulières, toutes préhistoriques. Du moins aperçoit-on que 'chemin', 'bras de mer', 'gué', 'pont' sont comme les variantes d'une signification qu'ils laissent reconstruire et que le problème ne concerne pas l'aspect sémantique du terme dans telle ou telle langue, mais qu'il se pose pour chacun d'eux et pour la famille entière dont ils sont les membres.

§8. Quand, dans la comparaison des termes d'un groupe unitaire, on se trouve en présence de développements de sens qui se distribuent en groupes tranchés, on est souvent obligé d'indiquer dans quelle direction le sens a varié et lequel des sens constatés a produit l'autre. Il faut bien alors se référer à un critère assez général et constant pour n'avoir pas besoin d'être à chaque fois justifié. Un des critères les plus usuels est le caractère «concret» ou «abstrait» du sens, l'évolution étant supposée se faire du «concret» à l'«abstrait». Nous n'insisterons pas sur l'ambiguité de ces termes, hérités d'une philosophie désuète. Il s'agit seulement de savoir si, même acceptés sans discussion, ils peuvent fournir un principe valable dans la reconstruction sémantique. Le meilleur moyen de les éprouver sera d'examiner l'application qui en a été faite—inconsciemment—dans un problème lexical d'assez grande portée. C'est le cas curieux d'une famille étymologique bien définie dans ses rapports formels, dont le sens se partage entre des notions très matérielles d'une part, morales et institutionnelles de l'autre.

Il s'agit du terme qui, en général, se rapporte à la 'fidélité' (*trust*) et qui, dans le Moyen-âge germanique, a eu une grande importance culturelle et sociale

(cf. *trust*, *true*, *truce*, etc.). L'unité du sens dans les formes germaniques ressort de leur simple énumération. En gotique, on a *trauan* 'pepoithénai, être confiant', *ga-trauan* 'pisteúesthai, se fier', *trauains* f. 'pepoíthēsis, confiance', *traustei* (d'après le gén. *trausteis*) 'diathḗkē, pacte, alliance'; de plus, v.isl. *trūa*, v.a. *trūōn*, v.h.a. *trū(w)ēn* 'avoir confiance', dérivés de **trūwō* dans v.isl. *trū* f. 'respect', v.a. *truwa* 'respect religieux, croyance', v.isl. *trūr* 'fidèle', au degré plein v.a. *trēowian*, v.h.a. *triuwen* 'se fier'; un dérivé **drou-sto-* donne v.isl. *traustr* 'de confiance, fort' et l'abstrait **draustyā* dans got. *trausti*, v.isl. *traust* 'confiance', v.h.a. *trost* 'fait de donner confiance, encouragement'; un adjectif **dreuwo-* dans got. *triggws*, v.isl. *tryggr*, v.h.a. *gi-triuwi* 'fidèle' et dans le nom v.a. *trēow* f., v.h.a. *triuwa* f. 'fidélité'. Mais hors du germanique, les termes apparentés portent un sens tout différent, qui est du reste représenté partiellement en germanique aussi. Ils désignent l'"arbre', parfois spécialement le 'chêne', ou le 'bois' en général: gr. *drûs* 'chêne', skr. *dāru*, *dru-*, av. *dru-* 'arbre, bois', *drvaēni-* 'de bois', got. *triu* 'bois, arbre' (et les formes correspondantes, angl. *tree*, etc.), gall. *derw* pl. 'chênes', v.si. *drěvo*, russe *dérevo* 'arbre', lit. *dervà* 'bois de pin'.

Comment organiser cette distribution de sens, 'arbre' d'une part, 'fidélité' de l'autre, dans un ensemble de formes qui autrement sont bien liées? Toute cette famille étymologique a été étudiée par H. Osthoff, dans un grand chapitre de ses *Etymologica Parerga* (1901) qui s'intitule significativement «Eiche und Treue». Il pose à l'origine de tout le développement morphologique et sémantique le mot indo-européen représenté par gr. *drûs* 'chêne', d'où procèderaient les valeurs morales impliquées dans *Treue* et *truste*. L'adjectif got. *triggws*, v.h.a. *gitriuwi* 'getreu, fidèle' signifierait proprement 'ferme comme un chêne'. Dans la mentalité germanique, le 'chêne' aurait été le symbole de la solidité et de la confiance, et l'image du 'chêne' inspirerait l'ensemble des représentations de la 'fidélité'. Depuis plus d'un demi-siècle, la théorie d'Osthoff passe pour établie; les dictionnaires étymologiques s'y réfèrent comme à une démonstration acquise.[7] On croirait donc avoir ici le type d'une désignation concrète évoluant en notion morale: une institution aurait pour origine un symbole végétal.

Mais dès le premier examen cette construction révèle ses failles. Osthoff en mettant le nom du 'chêne' au point de départ de toute la dérivation, admet implicitement—l'argument est essentiel pour sa théorie—que le nom du 'chêne' est indo-européen. Or tout le dément. C'est seulement en grec que *drū-* signifie 'chêne'. Partout ailleurs le sens est 'arbre, bois' en général: hitt. *taru*, i.ir. *dāru-*, *dru-*, got. *triu*, etc., v.sl. *drŭva* pl. En grec même, *dóru* s'applique à un arbre (Od. 6.167), au bois du navire (Il.15.410), au bois de lance et à la lance. Bien mieux, le sens de 'chêne' que gr. *drûs* a dans la langue classique est secondaire et relativement récent: un scholiaste (ad Il.11.86) savait encore que «les anciens appelaient *drûs* n'importe quel arbre» (*drûn ekáloun hoi palaioì . . . pân déndron*). Le terme générique pour 'arbre' a dénommé l'arbre le plus important, le 'chêne', probablement sous l'action des croyances attachées aux chênes prophétiques de Dodone. D'ailleurs le nom commun de l'arbre, gr. *déndrewon*, s'explique par un redoublement brisé, avec dissimilation, de **der-drew-on* (cf. lat. *cancer* de **kar-*

[7] Cf. Walde-Pokorny 1.804; Pokorny, *op. cit.* 214.

kro-), et repose sur **drew-* au sens d'"arbre'. Tout confirme donc que **dreu-* désignait l'arbre en général, et que le sens de 'chêne' a été acquis en grec seulement. Cette limitation a une raison: le chêne ne croît que sur une partie de l'aire indo-européenne, dans la région médiane de l'Europe qui va de la Gaule à la Grèce septentrionale, non au-delà vers l'Est; de fait il n'y a pas de nom indo-iranien du 'chêne'. Ainsi la démonstration d'Osthoff est atteinte dans son principe même; la signification qu'il croyait originelle se révèle tardive et limitée. Par suite la relation qu'il instituait entre les notions perd son appui principal.

Il faut pousser plus loin et dénoncer un vice de méthode dans l'argumentation entière. Les relations morphologiques et la distribution des formes n'indiquent pas entre les termes qui dénotent l'"arbre' et ceux pour 'fidélité' une relation telle que les seconds dérivent des premiers. Ils se répartissent également dans chaque langue et relèvent les uns et les autres d'une même signification, qui se laisse reconstruire à l'aide de l'ensemble des formes attestées. On doit poser la base formelle comme I **der-w-* II **dr-eu-*, avec le sens de 'être ferme, solide, sain'. Cf. skr. *dhruva-* (pour **druva-* contaminé par *dhar-*), av. *drva*, v.p. *duruva-* 'ferme, sain', gr. *dro(w)ón · iskhurón* Hes., v.sl. **su-dorva* > *sŭdravŭ*, r. *zdórov* 'sain', irl. *derb* (**derwo-*) 'sûr', v.pr. *druwis* 'foi' (< 'sécurité'), lit. *drútas* 'ferme, puissant', etc. Ici se placent naturellement les membres germaniques de ce groupe tels que got. *trauan*, *trausti*, etc. qui en dérivent tout droit et ont fixé en germanique la terminologie de la 'confiance'. Dès lors, c'est de cette commune signification que participe également la désignation de l'"arbre'. A l'inverse du raisonnement d'Osthoff, nous considérons que le **derwo-*, **drwo-*, **dreu-* au sens d'"arbre' n'est qu'un emploi particulier du sens général de 'ferme, solide'. Ce n'est pas le nom «primitif» du chêne qui a créé la notion de solidité, c'est au contraire par l'expression de la solidité qu'on a désigné l'arbre en général et le chêne en particulier: gr. *drûs* (gall. *derwen*) signifie littéralement «le solide, le ferme». Nous avons un parallèle en iranien, où 'arbre' se dit *draxt* (m. perse), *diraxt* (pers. mod.) qui remonte à av. *draxta-*, adjectif de *drang-* 'tenir ferme'. La conception romantique du chêne inspirateur de la fidélité fait place à une représentation moins singulière et probablement plus exacte: le nom **drū-* de l'arbre n'a rien de «primitif», c'est une qualification, qui, une fois attachée à son objet, en est devenue la désignation, et s'est trouvée séparée de sa famille sémantique; de là la coexistence de deux morphèmes devenus distincts, tels que *tree* et *true* en anglais. On voit ici combien est fallacieux le critère du «concret» et de l'«abstrait» appliqué à une reconstruction, et combien importante la distinction nécessaire entre la signification et la désignation.

§9. La différence de sens et la difficulté de la reconstruction atteignent un degré plus élevé encore quand les formes se répartissent en classes distinctes et grammaticalement inconciliables. Dans les cas envisagés jusqu'ici, on avait affaire à des formes dont le statut au moins ne s'opposait pas à une comparaison directe, le sens seul prêtant à discussion. Mais comment opérer quand les similitudes formelles sont contredites par des différences fonctionnelles? On peut mettre facilement en rapport des formes verbales et nominales réparties selon les principes de la dérivation. Peut-on rapprocher dans la même famille sémantique

des formes dont les unes sont des particules, les autres des formes verbales ou nominales, sans commun emploi syntaxique? Un tel problème est cependant posé par la coexistence de formes de séries différentes qui se groupent autour du terme indo-européen **pot(i)-* désignant le 'chef'. En essayant de le résoudre, nous répondrons à la question de méthode que ce cas soulève.

Un i.e. **pot(i)-* se présente à l'état libre dans skr. *pati-* 'chef' et aussi 'époux', gr. *pósis* 'époux', en composition dans skr. *jấs-pati-* 'maître de la lignée' (type indo-iranien très productif), gr. *des-pótēs*, lat. *hospes*, *compos*, lit. *viešpats* 'seigneur', got. *brup-faþs* 'bridegroom', etc. On y rattache aisément lat. *potis* et un ensemble de dérivés: *potior*, *possum*, *possideo*. Le sens, uniformément distribué, se définit comme 'maître, chef', avec un développement propre au latin et à l'italique vers la notion de 'pouvoir'. Mais il y a homophonie entre ce **pet-/*pot(i)-* 'chef' et une particule **pet-/pot(i)-* d'identité signifiant 'même, self': hitt. *-pet*, av. *-paiti*, lat. *-pte*, lit. *-pat*. Les deux ne se présentent pas toujours ensemble; le hittite n'a pas de forme de **pot(i)-* 'chef', et la particule semble manquer en sanskrit et en grec. Mais dans la majorité des langues l'un et l'autre apparaissent, sans toutefois qu'on discerne de liaison entre eux. La reconstruction d'un rapport sémantique doit nécessairement commencer par une décision de principe: laquelle des deux classes prendra-t-on comme point de départ? La question a été tranchée en sens opposés. Meillet jugeait qu'on devait procéder de **poti-* 'chef' et que la valeur de lit. *pats* '(lui)-même' résultait d'un emploi appositionnel, sur lequel il ne s'est pas autrement expliqué;[8] hypothèse qui n'est guère conciliable avec l'antiquité évidente de la particule. Plus vraisemblable, mais non exempte de difficultés, est l'opinion de H. Pedersen qui tire le sens de 'maître' de 'même', en alléguant non des preuves précises, mais des parallèles: il compare certains emplois de 'lui-même' désignant le 'maître de maison', tels que gr. *autós*, lat. *ipse*, dan. dial. *han selv* 'le maître', *hun selv* 'la maîtresse de maison', russe *sam*, *sama* 'barin i barynja'.[9] Mais tout ce que ces exemples peuvent prouver est que, dans une situation très particulière, qui est celle des familiers ou des domestiques, il suffit d'un pronom pour renvoyer au personnage d'autorité. Ainsi s'expriment à l'occasion des esclaves, dans la comédie grecque ou latine, mais non des hommes libres dans le langage solennel du culte ou de la poésie. L'emploi de *ipse* pour le maître de maison est un simple fait de «parole», il n'a jamais atteint le niveau de la «langue». Il est au surplus trop sporadique et récent pour rendre compte de formes évidemment archaïques et «nobles» telles que le couple skr. *pati/patnī*, gr. *pósis/pótnia*. On ne constate pas non plus que que cet emploi «ancillaire» de *autós*, *ipse*, etc. ait jamais produit une dénomination lexicale du 'maître' comme tel, ni une dérivation à partir de ce sens. Bref ces parallèles sont à la fois trop limités dans leur sphère et d'un style trop «familier» pour qu'on puisse y voir autre chose que des 'variantes de situation'; les pronoms *ipse*, *autós* peuvent occasionnellement désigner le

[8] Meillet, *Wörter und Sachen* 12 (1929) 18.

[9] H. Pedersen, *Archiv Orientální* 7.80 sq. et *Hittitisch* (1938) 77–8. Cf. déjà Schrader-Nehring, *Reallexikon* 1.216.

maître; ils n'ont jamais signifié 'maître' hors de leur contexte. Ils ne nous aident pas à retrouver la liaison des deux formes **pot(i)-*.

La manière dont les formes de chaque série se distribuent respectivement prête à observation. On notera que le hittite, dialecte archaïque à maints égards, possède seulement la particule *-pet* 'même' (*apaš-pet* 'lui-même, précisément lui'), il n'a pas trace d'une forme nominale telle que **pot(i)-*. Cela fait présumer que celle-ci a chance d'être secondaire. D'autre part les formes nominales du groupe de 'maître' ne se relient à aucune racine verbale; quand il y a une forme verbale, telle que skr. *patyate*, lat. *potior*, elle est clairement dénominative. Il s'agit donc d'une famille lexicale qui est entièrement et exclusivement nominale. Les termes en présence sont donc d'un côté une particule, de l'autre une forme nominale.

Il faut d'abord préciser la fonction de la particule *-pet*. Il y a dans les langues indo-européennes deux expressions distinctes de l'identité, qu'on peut illustrer par l'exemple du gotique, qui possède à la fois *sama* et *silba:* par *sama* 'same' s'énonce l'identité comme permanence de l'objet reconnue sous divers aspects ou en diverses instances; par *silba* 'self', l'identité comme opposée à l'altérité: 'lui-même' à l'exclusion de tout autre. Pour le dire en passant, la valeur d'insistance et de contraste inhérente à l'expression de la catégorie 'self' conduit à la signaler soit par référence à l'être corporel (d'où i. ir. *tanū-*; hitt. *tuekka-*; v.h.a. *leip*; fr. *en personne, en chair et en os*, etc.), soit par une dénotation emphatique, telle que le superlatif; d'où all. *selbst*, gr. *autótatos*, lat. *ipsissimus* (cf. *met-ipsimus* > a.fr. *medisme*, fr. *même*), sl. *sam* comme superlatif, etc., en tant que personnifications 'exemplaires' de la notion. C'est évidemment à la notion de 'self' que répond la fonction de l'enclitique hitt. *-pat*, lit. *-pat*, dont l'emploi est hérité: h. *apaš-pat* 'précisément celui-là, lui-même', lit. *ten-pat* 'là-même', *aš pats* 'moi-même', avec une valeur de superlatif développée en lituanien: *pàts pirmàsis* 'le tout premier'.

Dans cette fonction, la particule s'attache au pronom et il se produit alors une liaison sélective qui apparaît clairement en iranien, où *-pati* forme corps avec le réfléchi, av. *xᵛaē-paiti-* 'soi-même' et surtout le dérivé *xᵛaēpaiθya-*, v.perse *(h)uwāipašiya-* 'sien propre', dans la construction prédicative v.p. *(h)uwaipašiyam kar-* 'proprium facere, s'approprier', pour n'importe quelle personne, mais toujours pour une personne. De cet emploi on peut tirer l'explication du **pet/pot-* nominal, suffixé et nominalisé par *-i* dans **poti-*, qui signifiera la personne en propre, le ipse' avec une détermination quelconque. En effet le présent dérivé de *pati-*, skr. *patya-*, construit avec le datif, conserve le sens de 'être propre à': *āsutiṣ cārur mádāya patyate* litt. 'le breuvage agréable est propre à l'ivresse' (RV. VIII 1, 26), et av. *paiθya-* signifie 'avoir en propre' (et non 'être maître de'). Cette définition de **poti-* comme 'le ipse, l'être en personne' est conditionnée par la détermination qui en effet accompagne toujours le terme dans les expressions les plus anciennes: le **dems poti* (av. *dəng pati-*, véd. *dam-pati*, gr. *des-pótēs*) est littéralement 'le ipse de la maison, l'être même de la famille', celui qui *personnifie* la cellule sociale. C'est ce que nous transposons dans les

termes de notre propre culture par la traduction usuelle '*maître* de maison'. De là procèdent les autres composés, échelonnés dans la gradation territoriale, skr. *viś-pati-*, av. *vis-paiti-*, lit. *viešpats* 'celui qui est le ipse du **wik-* = maître du clan', etc.

Pour corroborer cette interprétation, nous trouvons deux indices. Le sens de lat. *hospes* (**ghos-pet-*), qui désigne aussi bien celui qui reçoit que celui qui offre l'hospitalité, s'explique bien comme 'le ipse' plutôt que le 'maître' de la prestation réciproque désignée par **ghos(ti)-* dans laquelle les deux membres sont partenaires égaux. En outre, il devient maintenant possible de lier la série des composés en **-poti* à une formation de même sens, mais de structure différente, qui appartient à l'indo-européen occidental. Saussure a autrefois attiré l'attention sur la formation curieuse des termes lat. *dominus*, *tribunus*, got. *þiudans* 'roi', *kindins* 'hēgemṓn', v.isl. *drottenn* 'prince', qui sont des dérivés secondaires en **-no-* des termes de base pour désigner des chefs: le *dominus* (**domo-no-*) est le chef de la *domus*, comme le *þiudans* (**teuta-no*) de la *þiuda*.[10] Si nous comparons la série des dérivés en *-no-* et celle des composés en *-poti*, nous voyons qu'elles sont parallèles et comportent des éléments communs: **domo-no-* et **dem(s)poti-*; **genti-no-* (got. *kindins*) et **gentu-poti-* (av. *zantu-pati*); un lat. **vicinus* serait le pendant de skr. *viś-pati*. Cette corrélation entre les termes en *-no-* du domaine occidental et les composés en *-poti*, qui prolifèrent surtout en indo-iranien, suggère qu'ils expriment la même notion. Or un dérivé en *-no-* ne peut guère par lui-même convoyer le sens spécifique de 'chef, maître'; **domo-no-*, **genti-no-* doivent signifier simplement 'celui de la *domus*, celui de la *gens*', c'est à dire en fait, celui qui la personnifie et en quelque sorte l'assume, qui agit en son nom et a autorité sur elle. Telle est justement la valeur que **poti* indique par lui-même: le personnage représentatif, un ipse investi d'autorité dans la fraction sociale, ce que nous appelons le 'maître'.

S'il en est ainsi, le fondement de l'histoire sémantique de **poti* 'maître' se trouve dans les syntagmes ou les composés dont **poti* est second membre. C'est ce que les faits confirment: skr. *pati-* 'maître' à l'état libre est tiré des composés où il a contracté son sens. Mais alors qu'en est-il d'une acception particulière du terme, celle de 'époux', attestée par skr. *pati-*, gr. *pósis*? Est-ce seulement le mari comme 'maître' de la femme? Cela satisferait une conception simpliste de la conjugalité indo-européenne, mais serait mis en défaut par le féminin *patnī*, *pótnia*. La dénomination se rapporte sans doute à d'anciens usages, sur lesquels un des composés, got. *bruþ-faþs* nous renseigne indirectement. La relation de *bruþ-faþs* 'numphíos, Bräutigam' à *bruþs* 'numphḗ' s'éclaire par les formes modernes *Bräutigam*, *bridegroom* (pour **-goom*), v.a. *bryd-guma*, où *-faþs* a été remplacé par le nom de l'"homme' (*-guma*), pour indiquer 'l'homme de la mariée', c'est à dire 'le partenaire masculin de la *brūti*'. Il faut se référer ici à de très antiques formules où les futurs conjoints sont posés l'un en face de l'autre comme partenaires d'une alliance: à Rome, *ubi tu Gaius, ego Gaia*; dans l'Inde, *amo 'ham asmi sā́ tvam* 'je suis celui-ci, tu es celle-là'.[11] De même dans le cas présent,

[10] F. de Saussure, *Cours de linguistique générale*, 4e éd. (1949) 309.
[11] Cf. *Language* 29 (1953) 259.

le *pati* et la *patnī*, le *pósis* et la *pótnia* (*-poina*) sont proprement le ipse et la ipsa de l'engagement qui les unit. C'est pourquoi le partenaire masculin de la *brūti* est dénommé comme **brūti-poti-*, où **-poti* a la même fonction que *-pet-* dans lat. *hospes*.

Dans cette restitution, on voit apparaître, comme facteurs décisifs dans l'histoire sémantique des deux morphèmes à identifier, la nominalisation de la particule *pel/pot-* en *-poti* et l'emploi de la particule avec un pronom pour souligner l'ipséité. Le développement des syntagmes (**dems poti*) et des composés est lié à la valeur institutionnelle des désignations ainsi créées, dans la structure propre de la société indo-européenne. L'homme qualifié d'un titre en **-poti* est à l'origine, non le chef ou le maître, mais le représentant de la division sociale.

Les faits latins méritent d'être considérés dans leur ensemble, parce qu'ils présentent, dans la variété des sens et des fonctions syntaxiques, comme un résumé du procès entier. L'importance prise en latin par le groupe de *posse*, *potens*, *potentia*, *potestas*, et la prédominance de la notion de 'pouvoir' dans les dérivés modernes ont obscurci aux yeux des philologues et des linguistes les relations qui articulent tout ce groups sémantique et en particulier les conditions où cette notion de 'pouvoir' s'est formée. Au point de départ, nous trouvons l'héritage de la particule enclitique (*mea*)*pte* qui sert à souligner ce qui est en propre, l'ipséité: suopte *pro suo ipsius*, *ut* meopte *meo ipsius*, tuopte *tuo ipsius* (P. Festus 409,1). On observera que *utpote* signifie non 'comme il est possible', mais 'comme il est propre (à la circonstance), comme il est naturel', et que le sens de l'adverbe comparatif *potius* 'plutôt, de préférence', superl. *potissimum* 'surtout' fait induire un *pote* 'juste, précisément, proprement', comme hitt. *-pat* ci-dessus.[12] Par là est introduite dans les formes nominales la valeur de 'qui est en propre', soulignant la possession comme 'propriété'. En effet *compos* signifie littéralement 'qui est mis en possession de', non pas seulement dans *compos sui* (ou *mentis*, *animi*) 'qui est en possession de ses esprits' ou comme nous disons, '*maître* de soi', mais aussi dans *compos culpae* (Pl. *Truc.* 835) 'qui est en possession de sa faute, qui s'identifie à elle, qui en assume la responsabilité', *compos voti* 'qui a la possession de son vœu, qui l'a fait sien' (= qui le voit réalisé), en connexion manifeste avec la valeur du composé av. *xᵛaē-paiθya-* 'proprius'. Tel est évidemment aussi le sens de *poti-* dans *possideo*, litt. 'occuper comme sien propre'. De l'ipse au dérivé 'proprius' se dessine un rapport qui va fixer le sens de 'possession'. Le présent archaïque *potio* signifie 'proprium facere, faire de quelque chose le bien propre de quelqu'un': *eum nunc potivit pater servitutis*, litt. 'son père a fait de lui la possession de la servitude' (Pl. *Amph.* 177). A cela vient s'ajouter le fait décisif que *potis* tend à se construire prédicativement; on voit ainsi comment *potis sum facere*, litt. 'je suis *à même* de faire, *ipse* sum qui faciam' devient 'je *peux* faire'. La notion de 'pouvoir' est alors constituée, un 'pouvoir' dépendant de la capacité distinctive de la personne, de son ipséité, et non de la nature humaine ou d'un concours de circonstances. C'est la dernière étape du procès

[12] Il serait tentant naturellement de retrouver cette particule dans la forme même de lat. *ipse*. Mais le rapprochement de *-pse* avec *-pote*, *-pte* crée une difficulté phonétique qui paraît insurmontable.

qui conduit d'une particule d'identité à la création d'un groupe nominal distinct, important et productif, et que les emplois, indo-européens aussi bien que latins, permettent de reconstruire avec quelque vraisemblance.

Dans ces analyses, qui visent surtout à illustrer quelques règles simples de méthode, nous avons fait choix d'exemples variés. Les problèmes considérés sont de complexité variable et de niveaux différents, situés dans la synchronie d'une même langue ou dans la perspective échelonnée d'une préhistoire lointaine. Ils ont été choisis pour leur valeur de types, et parce qu'il nous a semblé que chacun d'eux pouvait être amené à sa solution. Une méthode aux prises avec les difficultés d'un problème réel se laisse au moins juger sur les solutions qu'elle propose, tandis qu'à raisonner sur des conclusions acquises, on est sûr de gagner sans risque, et de n'enseigner que le connu.

Dans tous les cas discutés se trouve impliqué un problème de relation, et c'est par les relations qu'est définie une structure sémantique. Le lecteur averti discernera sans doute dans la démarche suivie ici les mêmes préoccupations qui se font jour dans d'autres parties de la linguistique actuelle, et mêmes certaines analogies dans l'objet de la recherche. Les considérations qui précèdent tournent autour d'une même question, qui est l'identification des traits distinctifs par opposition aux variantes: comment définir la distribution et les capacités combinatoires d'un «sens»; comment un sens tenu pour différent d'un autre peut ne représenter qu'une de ses variantes; comment la variante d'un sens se «sémantise» à son tour et devient unité distincte, tous problèmes qui se transposeraient immédiatement en termes de phonémique. Mais les notions sémantiques, beaucoup plus complexes, plus difficiles à objectiver et surtout à formaliser, étant engagées dans la «substance» extra-linguistique, appellent d'abord une description des emplois qui seuls permettent de définir un sens. Et cette description elle-même exige qu'on se délivre des fausses évidences, des références aux catégories sémantiques «universelles», des confusions entre les données à étudier et celles de la langue du descripteur. C'est peut-être dans le travail de la reconstruction que ces conditions sont le plus sévères.

Collège de France, Paris

ETYMOLOGY AND THE STRUCTURE OF WORD FAMILIES

YAKOV MALKIEL

Ever since etymology ceased to be a field for untrammeled speculation and sheer entertainment, attempts have been made to subject its hypotheses to certain tests of varying stringency. These controls, although rarely kindling imagination and conducive to new conjectures, can be used to good advantage in curbing a worker's excessively fecund inventiveness or in tipping the scales in favor of one of two equally attractive alternatives. In short, they tend to introduce the greatest possible number of rational elements into an operation essentially compounded of calculable ingredients and of imponderables.

Half a century ago, the application of the rigorous "phonetic" norm was regarded as the stiffest and decisive test, occasionally accompanied by a much looser semantic analysis. Linguistic geography brought to the fore the territorial criterion, culminating in the Italian neolinguists' sequence of areal norms, an innovation at that juncture wholesome and even necessary, if far too radically formulated. The *Wörter-und-Sachen* school opened up to the linguists the rich storehouse of accurate data relating to material civilization, inviting the stimulating (if sometimes misleading) check of patterns of names against the correlated patterns of objects named. Onomasiology, whether or not one inclines to grant it the status of a self-sufficient discipline claimed for it by some Central European supporters, closed another series of gaps by inducing workers to assemble regionally, socially, stylistically, or chronologically differentiated words of identical or similar meaning. The very difficulty of etymological solutions, given the scantiness of available records, seems to have redounded to the great benefit of scientific methodology, compelling the most ingenious scholars to search for fresh perspectives and avenues of approach, which in practice means to experiment with ever new classifications of data.

It may seem strange against the background of this avid search for new techniques of grouping that no systematic inquiry has, to my knowledge at least, yet been made into ways and means of extracting etymologically useful information from the configuration of word families. Modern linguistics stresses the fundamental kinship of inflection and syntax which traditional teachings used to separate quite sharply. Also it has become empirically known that word formation (that is, derivation and composition) which, with inflection proper, makes up morphology in the Indo-European languages, is best studied and practiced jointly with etymology: witness the output of researchers like A. Ernout and M. L. Wagner, who attack problem after problem of word origins and formative suffixes, simply because they have found the respective issues to be inextricably interwoven. In monographs and dictionaries alike—among the latter, Ernout-Meillet's and Walde-Hofmann's are more progressive in this respect than Meyer-Lübke's and even von Wartburg's—the consistent grouping of words by families has almost become standard practice. However, the graphic representation of such grouping in stemmata roughly comparable to those

used in the study of manuscript relationships has hardly gained recognition. In combing the immense volume of etymological literature, one finds fleeting allusions and casual hints at certain varieties of derivational and compositional hierarchy, but surely no attempt at organized typology.—If expediency dictates such steady combination of interests in practical research, it is perhaps legitimate to examine some facets of the underlying theoretical problem.

There exist in Indo-European certain categories of words from which only a severely limited number of derivatives, if any, are on record: conjunctions, prepositions, and some pronouns. Characteristically, where an isolated word of this kind offers etymological difficulties, these usually represent very serious obstacles. Half a century after E. Richter's substantial contribution scholars are still debating the extent to which Fr. *à*, Prov. It. Sp. Ptg. *a*, and their congeners reflect Lat. *ad* and *ab(s)*, *ā*. It recently cost J. Svennung and, independently, E. Gamillscheg each a painstaking study of phraseological detail to demonstrate that a word as common as It. *da* is a merger of *dē* and *ab*. In Ibero-Romance, Ptg. *até* 'until' has finally been accounted for by E. K. Neuvonen (in his recent study of Old Galician Arabisms), but its Spanish counterpart *hasta* (beside OSp. *ata*) and Sp. *desde* 'from', *alrededor* 'around', and *hacia* 'toward' all continue to offer resistance to the etymologist, at least at one point of their trajectories. The tracing of Rum. *tare* 'big' to Lat. *tāle* 'such' (colloquially used, it is assumed, like Fr. *gros comme ça*, Engl. *that big*, with an appropriate gesture) still leaves some scholars unconvinced, despite the striking parallelism of *care* < *quāle* and the clue offered by the general decay of *magnus* at the Romance stage. An important, if not chief, reason for the margin of doubt in each case is the lack of collateral equations. Lat. *bōs bouis* 'ox' does not lend itself to smooth connection with its putative descendant, Rum. *bou*, but derivatives like *boar* 'cowherd' < *boāriu*, in addition to Vulgar Latin forms such as *bobus* (Chiro), lend it the necessary measure of support.

Let us, first of all, distinguish between variants of given forms and genuine satellites (derivatives and compounds). The variant, whether phonological or morphological, is the etymologist's most trusted ally, be it only for its rôle in cautioning him against facile solutions: OFr. *derver* slows down the pace of the conjecturalist who comes up with another glib explanation of the crux Fr. *rêver*, and the twenty-four Old Spanish variants of the toponym *Madrid* which Menéndez Pidal has brought to light make a shambles of all earlier Latin, Greek, Gothic, and Arabic derivations. A formation lacking in significant variants is at once recognizable as one of potential difficulty in etymological reconstruction. The tag "significant" is here attached to such variants as permit the explorer to narrow down the choice, to reach the final decision, or at least to discover some additional evidence. Thus, the fact that It. *tir-are* contrasts with Sp. *tir-ar* and with Fr. *tir-e(r)* is irrelevant, since the radical remains unchanged and the normal distribution of verbal morphemes in the three languages adds nothing of specific value to the existing information; similarly, comparison of Fr. *piquer*, It. *piccare*, and Sp. Ptg. *picar* fails to lead us beyond the reconstructed schema **pīccāre* 'to prick', in the absence of any semantic clue. The case of the much-discussed adjective *bravo* is slightly different, inasmuch as in Spanish and

Portuguese it means 'wild, tempestuous', applying to the untamed bull or to the roaring sea; in Italian it stands for 'efficient, good at performing a task', in reference to well-behaved children (hence borrowed Germ. *brav*) and to skillful performers (hence our *bravo* as an accolade, transformed into an interjection in Russian, German, and other languages); the French counterpart *brave* signifies 'courageous', a meaning echoed by Engl. *brave*. Since formal differences are more dependable guides in reconstruction than are contrasts in semantic shades, most etymologists, if faced with a choice, would prefer to settle on three sufficiently differentiated forms, at the cost of identity of meaning, rather than on the actually more common reverse distribution of opposition and coincidence. Since we cannot force the record, the wisest decision in this particular instance is to hold out hopes that, at least, the richly diversified meanings, if traced to the earliest ascertainable stage and neatly localized, will in the foreseeable future point a way out of the present embarrassing impasse.

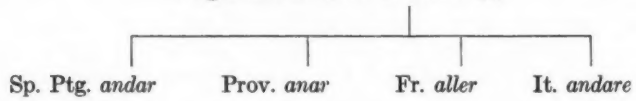
There exist other blind alleys in etymology due to the absence of significant variants. In analyzing *trobar* (*trouver*, etc.) which, in an extensive territory, means or meant either 'to find' or 'to compose' (verse, music, cf. *troubadour* < OProv. *trobador*), scholars still face a classic alternative, pointed up by the celebrated controversy between A. Thomas and H. Schuchardt (the sympathies of J. Jud and others have lately swayed in the direction of Gr.-Lat. *contropāre*). *Matar* 'to kill', anciently also *amatar* 'to extinguish', both peculiar to Ibero-Romance alone, are of Latin background according to some (a single authentic instance of **meitar*, **mechar* would suffice to settle the issue in favor of *mactāre*), of Arabic provenience according to others (who may find it hard to explain away the same semantic dichotomy in Lat. *tūtārī* 'to protect [the home by extinguishing the fire]' ~ Fr. *tuer* 'to kill').

Assuming that, luckily, two or more variants diverging in significant details are on record, how do we go about classifying them, if their rough chronological and geographical distribution is, genetically, inconclusive? The aim should be to arrange these variants in such a way as to reconcile their assumed sequence in time and space with the known hard facts of historical grammar, which, to be valid, must be based on reliable equations. As usual, we proceed from the better known to the less well known. If, from the rivalry of variants distinguished by closely allied formatives, we conclude that a prefix, infix (H. Lausberg prefers "interfix"), or suffix change has taken place, the entire question revolves around our ability to determine which formative, at the critical moment of the shift, was spreading at the expense of which others. OSp. *posfaçar*, *porfaçar*, *profaçar* 'to mock, upbraid' are all three unanalyzable at first sight. Of the competing prefixes or quasi-prefixes involved, *pos-*, from Lat. *post-*, is found either in a few learned borrowings (*posponer*) or in typical relics in which its vowel sometimes undergoes changes (*pospierna* beside *pescuezo*). Having been unproductive in the post-Isidorian vernacular, it cannot possibly have tended to evict *por-* and *pro-*, both prefixes of demonstrable longevity and continued vitality. If we add to our roster of verb forms the Old Galician substantive *posfaz* 'mockery', which smoothly resolves itself into *post faciem* (*rīdēre*) 'to laugh behind one's face' (= 'behind one's back'), *posfaçar* turns out to be a newly coined verb

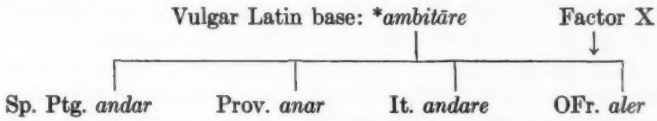
based on a Latin adverbial phrase, with *por-* and *pro-* secondarily encroaching upon its progressively less meaningful prefix.

A similar procedure recommends itself when formations suspected to be variants clash in features affecting the radical itself: rival consonant clusters, monophthong vs. diphthong, some manner of abridgement or condensation (apheresis, syncope, haplology, apocope) vs. full maintenance of the original syllabic structure. The likeliest succession of events within the history of the pertinent segment bids fair to lead us to the solution of the entire etymological tangle. The Hispanic products of Lat. *macula* 'mesh, blot' (that is, the cognates of Fr. *maille* and It. *macchia*) include Gal.-Ptg. *mágua* 'defect, disease, scratch, blemish' (later 'pain, nostalgia'), OSp. *mágula* 'blow' (cf. *magullar* 'to bruise, mangle'), Sp. Ptg. *mancha* 'blot, stain', Sp. *mangla* (Ptg. *mangra*) 'mildew, blight', Extr. *mangria* 'sticky, blackish crust', OSp. *mandra* 'fold' (unless it is a Hellenism), Ptg. *malha* 'net, fold, shepherd's cabin' (Arag. *malla* 'spot', cf. Sp. *majada* 'fold, herd'). In this conspicuously ramified lexical family the simultaneous impact of tendential syncope and nasal insert complicates the situation. *Mangr(i)a* is a slightly erratic, *mandra* a completely aberrant form, both of which can be linked to the remainder of the family only as a result of their semantic normalcy. The core of the variants falls into a clear-cut, though admittedly complex, pattern.

A workable definition of "variant", whether phonological or morphological, may perhaps be best arrived at inductively. Consider Fr. *al(l)er* 'to go' (thus from the earliest texts) vs. It. *andare* 'id.' (which, dialect geography avers, must have spread from the north), near-synonymous Sp. Ptg. *andar* 'to walk' (extant from the earliest records), and OProv. *anar*. To the solution of this narrow problem presumably over one thousand printed pages have so far been devoted. Most of this output is worthless, through disregard of a single and, one should think, simple matter. It. *andare*, Sp. Ptg. *andar*, and Prov. *anar* can all quite smoothly be subsumed under the single archetype West.-Vulg.-Lat. **ambitāre*, one of the few hypothetical bases that cannot, on good grounds, be impugned. Admittedly, Fr. *aller* does not match this schema. This partial, one may say, regional departure from an otherwise pervasive pattern has, again and again, aroused doubts as to the general validity of **ambitāre*. Behind these misgivings lingers the belief that an inferred base, to be acceptable, must equally well fit whatever passes as its product; a belief linked to the desire to project as many evolutionary phases as possible into the common ancestral language. This is the same kind of "telescoping" over which the older and the younger Indo-Europeanists seeem to have parted company; witness É. Benveniste's reaction to R. G. Kent's meritorious grammar of Old Persian. Schematically, the approach of the earlier school of thought may be expressed thus:



Yet, knowing as we do that northern Gaul's provincial Latin tended to be advanced and relatively unstable rather than retarded and stagnant, why not concede that this one erratic offshoot *aller* found in an area of innovations has been deflected from its normal course by an unidentified (perhaps unidentifiable) extraneous influence, say, the blend with another word, indigenous or Latin? We might call this preliminary operation DISENGAGING abnormal variants which obstruct the path. The schema, thus corrected, gives a different impression (the special minor problem of the source of Prov. *–n–* is here disregarded):



An area of doubt subsists, but it has been substantially narrowed down; in reference to northern Italy, to France south of the Loire, and to the entire Iberian peninsula, **ambitāre* may be used at will in further reconstructions (as it has been recently, in the derivation of Sp. *andén* 'gallery, platform', orig. 'orbit' beside *andana* from Lat. *ambāgine*, on the assumption of early contamination by *andar*); the chances of eventually identifying X have increased after it has been brought into sharper focus; the singling out of northern Gaul as the typical zone of sweeping change gives a more vivid and realistic picture of the delicately shaded provincial varieties of Late Latin.

By variants we shall, then, ordinarily mean those that can, without stretching or straining the facts, be subsumed under a defensible archetype. Such by-forms as do not lend themselves smoothly to this process we had best place in temporary reserve as not directly helpful at the initial phase of the analysis. The moot question as to whether they represent distant relatives, disguised through associative interference of another lexical family, or are genetically unrelated and may be characterized as deceptive near-homophones, must, as a rule, be postponed until the closing stage of the inquiry.

As long as the relationship between radical and formative is normal (that is, if the morphemes indicative of agent, action, place, time, tool, accumulation, progeny etc. are joined to the radical in the expected manner), the etymologist rarely receives any conspicuously valuable clue. But once a worker has detected a trace of anomaly which is not a plain instance of extended scope ("Funktionsüberschreitung," like Fr. *devinette* from the verb *deviner* rather than from a substantive, or Fr. *bonbonneuse* 'factory worker wrapping candy boxes' from the substantive *bonbon*, because the use of *bonbonnière* has already been preëmpted), it is tactically rewarding to concentrate all available resources on this one vulnerable spot in an effort to force a breakthrough.

The Hispanic verb *cansar* 'to tire' (as distinct from its Italian homophone) is of disputed parentage (probably, it represents a cross of Lat. *quassāre* and Gr.-Lat. *campsāre*). Whatever its ancestry, one of its derivatives, Sp. *cansino*, a rustic adjective meaning 'tired, worn out' and used in literature only at intervals, exhibits a striking abnormality: the addition to a typically verbal stem of the no

less typically nominal suffix *-ino*. Lat. *-īnus* served, for instance, to derive adjectives from names of animals: *equīnus*, *lupīnus* (a function traces of which persist in Sp. *palomino* 'young dove'); Gal.-Ptg. *-inho* and It. *-ino* are widely employed as general diminutive suffixes. However wide the extension of its original scope, Sp. *-ino* has nowhere divested itself of its basically nominal character. A lucky accident comes to our rescue: in a single early charter we find the proper name *Campsino*, clearly a syncopated variant of the familiar and, structurally, transparent *campesino* 'peasant, tiller', based on *campo* 'field'. In the course of the long struggle for syncope in preliterary Spanish, the doomed alternant *campsino* evidently went astray, broke loose from its family, and joined another group of similar appearance, clustering around a verb (*cansar*) whose meaning *cansino* finally adopted. *Caballo cansino*, at the outset presumably a 'horse working in the fields', gradually became a 'horse exhausted by hard work in the fields', later simply a 'worn out horse'. But for the precious intermediary link *Campsino*, with the unstable cluster *-mps-*, we should never have quite so securely traced *cansino* to its source; at best, some misgivings might have arisen from our knowledge that radical and suffix do not match, as if something highly irregular had here, rather clumsily, been patched up by untutored speakers.

If we let ourselves be guided by unusual or oddly employed suffixes, our chances of solving the riddle of the nuclear formation, *cansar*, may sharply increase. It. *(s)cansare* 'to shirk, shy back, withdraw, avoid' clearly perpetuates Gr.-Lat. *campsāre*, a term of navigation ('to double or round'), but opinions are divided on the origin of Sp. Ptg. *cansar* and the discussion, despite a brilliant roster of participants, has not made much headway in the last few decades. The clue is given by Sp. *cansancio* (OSp. *cansaçio*), OPtg. *cansaço* 'fatigue'. The suffix *-açio*, *-aço* here undoubtedly perpetuates Lat. *-ātiō*, not *-āceu*, and the entire formation echoes *quassātiō* 'break', transmitted, as a medical or surgical term, in the nominative (cf. Sp. *bullicio* < OSp. *bolliçio* 'bustle, uproar' < *bullītiō* 'ebullition' and the like). Hisp. *cansar* must, then, rate as a merger of *campsāre* (with *-mps-* reduced to *-ns-* at an early date) and *quassāre*, the frequentative of *quatere* (cf. Fr. *casser*), to which the blend owes the derivative *cansancio*; a merger perfectly understandable against the background of protracted Vulgar Latin wavering between *-ns-* and *-s-*, whatever their sources (cf. *thensaurus* beside Gr.-Lat. *thēsaurus*, whereas Fr. *trésor* shows another deflection; OSp. *fonsado* 'army' < *fossātu*, and similar instances of hypercorrection). The infrequent suffix *-a(n)cio* has been the keystone, though surely not the sole support, of our entire edifice.

Our horse sense tells us that Fr. *mensonge* and *mentir*, It. *menzogna* and *mentire*, Hisp. *mentira* and *mentir* are cognates, yet no French abstracts in *-songe* from verbs in *-tir* are otherwise known, nor has any Spanish or Portuguese verbal developed through the addition of *-a* to the full infinitive form. Again, anomaly is a powerful stimulus for sharply focused meditation: two scholars have recently come up with (mutually not quite exclusive) rival explanations, one of which strives to account for the departure from the norm by associating Vulg. Lat.

ment(ī̆l)iōne 'lie' with *daemonia* 'demons', against the background of patristic writings, whereas the other reckons with the early polarization of *uēra* 'true things' (and *iūra* 'rights', later 'oath') and **mentira* 'false things'. True, such demonstrations, to carry conviction, require an unusual amount of data and, as a result, attract few workers: while it is easy to match Fr. *soin* and *soigneux* or to set up the triad *besoin* ~ *besogne* ~ *besogneux*, it is difficult in the extreme to formulate the relation of *soin* and *be-soin:* the finding of a felicitous formula for this morphological relation will indubitably coincide with the discovery of the definitive etymology. The same applies to Sp. *marfil*, Ptg. *marfim* 'ivory', whose initial syllable has proved impervious to analysis (*-fil* reflects Ar. *fīl* 'elephant'). In conclusion, the degree of triviality in the relation between radical and formative (or a second, hidden radical, if *marfil* should turn out to have been, originally, a compound) is in inverse ratio to the potential usefulness of the morphological data involved to the etymological inquiry.

Sometimes similarity of derivational contour in two word groups suspected of kinship can serve as collateral proof that one group is the descendant of the other or that they are both subfamilies of a larger group. Thus, OSp. *(d)espedir* 'to send away, discharge' (a soldier, a vassal) has for seventy years been considered a product of *expetere;* this explanation, originally advocated by Cornu, can now be shown to have been erroneous, and one of the strongest arguments against it is the obvious link between OSp. *(d)espedimiento* and the Latin series *expedīmentum, impedīmentum, praepedīmentum* and the lack of any comparable formations within the *petere* family. Examples of similar configuration of cognate word families, cutting across the frontiers of, say, Old French, Old Spanish, and Old Italian, are too common to warrant illustration. More noteworthy are parallel rather than exactly identical offshoots: the derivation of OSp. *calaño* 'similar' from *aequālis* (via **aequālāneu*) is indirectly supported by It. *gualivo*, Raet.-Rom. *gualif* (Grisons), *ualif* (Friuli) 'plain, level, smooth', Ptg. *galivar* 'to cut, chip, square', all of which posit **aequālīuu:* the same force (escape from homonymy?) that in one province presided over the addition, to *(ae)quālis*, of *-āneu* could elsewhere have favored the addition of *-īuu*. Where a single verb, with the aid of stem alternants, has split into two, mutually supplementary derivatives may bear out the original oneness of the lexical family. Thus, Sp. *cundir* 'to spread', *cundidura, cundeamor*, dial. *cundi(o)* 'laborer's meal' and OSp. (dial.) *condir* 'to season', *condim(i)ento, condi(d)o*, and *condidura* fit like two fragments of a broken vase and show beyond the shadow of a doubt that Lat. *condīre, condīmentum* (and not Goth. **kundjan*, open to criticism on all counts) are the common starting points. The proof is that one can freely separate any morpheme used in the first series and attach it to one of the words included in the second series, and viceversa.

Much as infection may, from a limb, spread to the remainder of the body, so in lexical matters a given anomaly may have originated not at the core, but near the periphery of a word family (using "core" and "periphery" in terms of derivational hierarchy, to the exclusion of geographic localization). Take Sp. *acechar* (beside OSp. *assechar*, OPtg. *asseitar*) 'to stalk', traceable to Lat. *assectārī*

'to pursue' (related to *sequī*). The development from Latin to the medieval vernaculars, ordinarily the span hardest to account for, happens here to be entirely transparent, but the modern Castilian form [aθetʃár] has puzzled many workers. A careful inventory of derivatives shows the important rôle once played by *assechança* (OPtg. *asseitança*), and knowledge of assimilatory processes explains the tendential shift of Late OSp. *assechança* to *açechança* [atsetʃantsa], through anticipation of the two affricates, a phenomenon of which there exist numerous examples. Once *açechança* had been admitted alongside *assechança*, the [ts] > [θ] infiltrated into the center and thence into other branches of the word family, giving rise to *acechar* and *acecho*. (Ironically, the only derivative that, in the final account, failed to generalize the secondary form was the abstract in *-ança* which had given birth to it; this paradoxical situation was due to special circumstances, irrelevant in this context.)

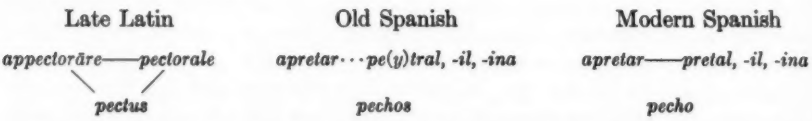
Within the bounds of a single word family, the shift of the center, under certain circumstances, may be observed. The complete eclipse of the center is seen in the case of Fr. *bonheur*, *malheur*, *heureux*, *bienheureux*, *malheureux*, as a consequence of the decline of OFr. *eür* < *a(u)guriu*; but this change entails no special difficulties for the etymologist. The displacement of a center seems more interesting than its total disappearance. Take a common Romance adjective, surrounded by abstracts ending in typical suffixes (say, Sp. *-edad*, *-ez*, *-ura*) and accompanied by a rare verb which, at first, fails to arouse suspicion on derivational grounds. From the start, the scholar's attention centers about the adjective which, containing no discernible formative and being, within the family, a leader in terms of range and frequency, appears as the obvious nucleus of the group. If, in nine out of ten cases, such an adjective yields smoothly to etymological analysis, so much the better. But supposing that it puts up resistance, should not scholars weigh the possibility, however remote, that the verb, at present relegated to the periphery, might, in the distant past, have represented the center? If this were true, the seemingly primary adjective would, in fact, be classifiable as a regressive formation, that is, a derivative deceptively arrived at without the addition (and, in extreme cases, with the outright loss) of a morpheme. Such an assumption has merit if the verb is readily traceable to an etymon (which the intractable adjective, its reputed base, was not). The new hypothesis can be strengthened by the statistical demonstration that, contrary to earlier belief, the adjective which at the start arrested our attention lacked a margin of frequency over the verb at the initial stages of its growth. Take Sp. *terco* 'stubborn', surrounded by *terquedad*, *tercura*, and similar inconspicuous offshoots, an apparently familiar picture which, in reality, disguises an utterly different situation. No satisfactory etymology for *terco* has been proposed by those who have treated it as a primary adjective. If, after some search, we discover that *terco* was absent from the best-known oldest texts, this testimony of the record is a warning to be heeded. Aligned with Gal. *terque* and with OCat. *enterch*, the Castilian adjective at once suggests the rare verb *entercar* as the most plausible starting point for the growth of the entire family. The conjecture becomes a certainty when the obsolete non-syncopated variant of the verb, *enternegar* 'to harden', is traced to Lat. *interne-*

cāre (with intensive *inter-*) 'to kill, destroy'; applied to rural produce, 'to freeze, harden'. A royal road leads from 'hardened' to 'stubborn'.

Still other changes within the confines of a lexical family may sometimes obscure an etymological issue. The varying distances between its individual members play an important part in this process of concealment. The splitting up of a single family into two or more independent entities is a familiar phenomenon; the definitive separation is preceded by a slight fissure, granting semi-independence to an individual member or to a subgroup. Less usual is the disappearance of the looming menace of cleavage through an unexpected new coalescence, much as, to quote A. Martinet, in the formation of dialects there seems to prevail a delicate balance between divergent and convergent currents. This healing of an incipient rift is, of course, conceivable only where the distance remains relatively short; that is, where on both sides the same or almost the same interplay of semantic shades continues and where the obstacle of a minor differentiating sound feature may be gently removed. In Late Latin, for instance, *pectus -oris* 'breast, chest', *pectorālis* 'belonging to the chest' (speaking of a bone or of a garment), and *appectorāre* (Isidore) 'to press against the chest' all three plainly belonged together. In Castilian one finds from the earliest texts no trace of the *pectore* stem (which lingers on in a small Gallo-Romance dialect group). *Pectus* alone has persisted: its product *pechos* (reinterpreted as a plural) through gradual advance has drifted away from *pe(y)tral* 'poitrel, breast-leather', the direct descendant of *pectorāle*, flanked by its new variants *petril* 'parapet, breastwork' and *petrina* 'belt, girdle'. On the other hand, *appectorāre*, as a consequence of its blend with the powerful near-synonym *premere* 'to crush, squeeze', has not evolved in the direction of its natural goal, **apeitrar*, but has assumed the form *apretar*, which we are privileged to find corroborated by a precious mid-tenth century record. From whatever indirect evidence can be pieced together we gather that *pechos*, by the year 1000, was no longer closely associated by naïve speakers with either *pe(y)tral* and its morphological variants or *apretar* and its offshoots, including *prieto*. But, what with the singular mobility of the *r*, what with the subsisting similarity of the imagery involved, the ties between the *pe(y)tral* and the *apretar/prieto* group were at no time definitively severed. By the year 1600, with the increased importance of *apretar* and the simultaneously lowered rank of *petral* and *petril* (in consequence of changes in material civilization), the new triad *pretal*, *pretil*, *pretina* began to emerge in all but the most conservative peninsular dialects and throughout the Spanish-speaking New World. This group, after a separation of seven centuries by a conservative estimate, was reunited with *apretar* and its satellites. Almost the entire gamut of shifts and oscillations involved in this complicated process happens, fortunately, to be amenable to observation. Had the protracted wavering between the stem alternants *pret-* and *pert-* been methodically observed from the outset, many otiose statements on the etymology of *apretar*, also of Sp. *prieto* 'black, tight', Ptg. *preto* 'black' and of the Portuguese preposition and adverb *perto*, anciently *préto* 'near, close by', might have never appeared in print.

Schematically, the relations, at the characteristic turning points, might be

represented thus, with straight lines indicating solid links and dotted lines suggesting loose association:



The terms "cross," "blend," "amalgam," "contamination," which most of us use haphazardly, are subject to caution like all metaphors introduced into the language of science. The measure of their usefulness is that they give simple graphic contours to situations that are intricate in the extreme. This purpose is served well as long as the individual word remains in the focus of interest. The proposed shift to the word family as the most fitting unit in lexical research entails the need for different imagery. We can muse that, if we choose to visualize the total vocabulary of a given language as a field (a metaphor no longer new) and to symbolize the lexical families by circles of varying radius, depending on the number and perhaps relative frequency of the derivatives and compounds on record, these circles should be understood as being sometimes widely separated, occasionally adjacent, and in more cases than is usually anticipated, overlapping. Almost any word-family of reasonable proportions, if studied at sufficiently close range, shows at least vestigial interlocking with other families. A measure of contamination between families of similar form or similar meaning is the rule rather than the exception; and where such changes occur as diffusion (in the perspective of space) or obsolescence (in the perspective of time), the ensuing insecurity inevitably accelerates the rate of contamination. Reverting to our image, the area common to both overlapping circles may be small or large; in exceptional instances, the circles coincide entirely or almost entirely. The extreme case of total coincidence would mean that no single derivative is excluded from contamination. These symbolic areas would have no bearing whatever on the respective geographic territories.

Systematic classifications along the lines here suggested may be a thing of the future. Just as scrupulous dictionaries are likely some day to accompany each entry by a small map, in tribute to the accomplishments of linguistic geography, so etymological dictionaries, a century from now, bid fair to arrange lexical families in the form of derivational stemmata or constellations, to give the reader an instantaneous impression of the pattern of relationships involved; and, speaking of the interpenetration of "neighboring" lexical families, scholars may resort to some such imagery as overlapping geometric figures. But the systematic study of derivational and compositional delineations of word families, even if sanctioned by scholarly consensus, is bound to remain a mere adjunct to established analyses, not a complete reversal of earlier practice.

University of California

INTERNAL RECONSTRUCTION OF PRE-INDO-EUROPEAN WORD-FORMS[1]

CARL HJ. BORGSTRÖM

1.1. There are at least two recognized procedures for tracing the history of a language or a group of languages: comparison of various documented stages of one linguistic tradition, and comparison of languages which are believed to have been dialects of one language in the past. The latter method has come to be known as the comparative method *par excellence*.

There is, however, a third set of procedures which aim at reconstructing earlier stages of a language when neither older texts nor related languages are known. A suitable term for this method is *internal reconstruction* in contrast with the better known comparative reconstruction. Examples are F. de Saussure's analysis of PIE long vowels into short vowels followed by a '*coefficient sonantique*', and W. Streitberg's attempt at an explanation of PIE lengthened vowels ('*Dehnstufe*').

1.2. The theoretical foundation of internal reconstruction lies partly in synchronic, partly in diachronic linguistics.

1.2.1. Structural linguistics aims at obtaining simple patterns by reducing the number of entities in each pattern to the minimum prescribed by the principle of relevancy. When apparently different entities are in complementary distribution, they are described as variants (allomorphs, allophones) of one entity. A notable simplification of morphemic patterns is obtained when the distribution of allomorphs is phonemically definable, as in the case of the English plural endings /-s/, /-z/, and /-iz/. Even different morphophonemic changes may sometimes be reduced to a common pattern: the interchanges /a/—/e/, /o/—/ö/, /u/—/ü/, /au/—/oü/ in German nouns (sing.—plur.) and adjectives (positive—comparative) are all instances of the pattern /non-front vowel/—/front vowel/. Now it happens that such simplification can be achieved only by changing certain parts of the given system, e.g. the number or distribution of its phonemes. Thus F. de Saussure found that most of the PIE series of vowel-gradation (*ei—oi—i*, *en—on—ŋ*, etc.) were cases of the one interchange *e—o—zero;* but a series like *ē—ō—A* was refractory unless he changed it into *eA—oA—A*, where *A* played the same part as *i*, *n*, etc. in the other series.[2]

[1] Abbreviations: PIE = Proto-Indo-European, as partially reconstructed by the comparative method; PreIE = any earlier stage of PIE which may be partially reconstructed by internal methods. NTS = *Norsk Tidsskrift for Sprogvidenskap* (Oslo). RDV, see 4.6.

Transcription: h_1, h_2, h_3 stand for laryngeals, h_2 for the *a*-coloring, h_3 for the *o*-coloring one(s); h_x when the identification is doubtful. For *ā* see 3.1; the asterisk is omitted before reconstructed words spelt with *ā*.

[2] *Recueil des publications scientifiques de F. de Saussure*, p. 127. This hypothesis reduced several grammatical paradigms of PIE to common patterns, cf. *ibid.* pp. 137f. and 224f. It is the basis of the later "laryngeal theory"; cf. J. Kuryłowicz, *Etudes indo-européennes* I, p. 27f.

1.2.2. A diachronic justification of internal reconstruction lies in the observation that some complicated patterns, particularly morphophonemic interchanges and certain allomorphs, have developed from simpler patterns of earlier periods. Thus the interchange in Eng. *man—men, foot—feet* is explained, i.e. reduced to a simpler pattern, when we know that it is due to the earlier presence of an ending **-iz* in some forms of the plural, and that unstressed *i* caused a phonemic change of the preceding vowel. It is therefore legitimate to ask for such explanations of complicated patterns even in languages whose history is not known, and to obtain them by assuming that certain phonemic and morphemic changes have taken place. The changes assumed must not contradict the principles of diachronic phonetics, phonemics, or morphemics.

1.3. On the other hand, we observe that complicated patterns may be simplified in the course of the history of a language, and that phonemes or morphemes may be lost without leaving traces. Since there is no way of reconstructing lost complications or entities, all reconstructive procedures are likely to yield an over-simplified picture of the reconstructed system.[3]

1.4. Internal reconstruction need not always follow strictly the same course. Here are some special cases.

1.4.1. A certain pattern, e.g. a rule for the distribution of entities, which is not motivated in the given system, may be motivated, though not rendered simpler in itself, under certain systematic conditions which may then be assumed for an earlier stage of the language. For example, the distribution of consonants in PIE themes may become motivated by the assumption that themes were originally monosyllabic, cf. 3.3.

1.4.2. Alteration of one part of a linguistic system will often have consequences for the functioning of other parts of the system. Having reconstructed part of an earlier system by altering something in a given language, one may sometimes draw further conclusions about the structure of the earlier system. An instance of this type is the conclusion drawn in 3.1 about PreIE vowels.

1.5. A short description of how internal reconstruction works would run as follows. A complicated or unmotivated pattern in a given language may sometimes be reduced to a simpler pattern, or be motivated, by the assumption that certain regular phonemic or morphemic changes have taken place in the past; thereby parts of an earlier system are tentatively reconstructed. The functioning of entities within this system should be determined as far as possible.

2.1. The terms 'word' or 'word-form', 'root', 'theme', and 'stem' play such important parts in what follows that it is necessary to indicate how I intend to use them.

2.2.1. *Words* are utterance-segments consisting of one or more morphemes. Assuming that there is a procedure for the demarcation of morphemes, it is

[3] Cf. the correct remarks by A. Martinet, *Word* 9.267 (1953), about the dangers of striving after maximal simplicity in reconstructive linguistics. The tendency is inherent in internal reconstruction and must be counteracted by the principle that no reconstructed system should be treated as a complete linguistic system.

possible, I believe, to formulate a set of distributional rules as a procedure for the demarcation of words.[4]

2.2.2. In some—perhaps all—languages, the unity of words as defined through the distribution of morphemes is strengthened by rules for the distribution of certain phonemes, allophones, or prosodemes within words. For instance, in Latin, Polish, Finnish, the distribution of accents is automatic with reference to word boundaries except in so far as certain words may or may not have an accent. The concept of 'word' is important for the understanding of many diachronic developments, cf. 3.5.

2.3. The following definitions apply to Proto- and Pre-IE only. A morpheme which may, without a derivational suffix, take verbal inflexional endings (and usually even nominal endings) will be called a *root* or *theme*; a *root* is such a morpheme which contains two consonants (e.g. **b'er* 'carry', **wel* 'will'), while a

[4] The following tentative rules, which are based on a few languages only, may serve to indicate what I mean. A morpheme is here understood to include its allomorphs; the stressed and unstressed forms of a morpheme count as different morphemes, e.g. in Eng. *blâck bîrd* and *blăckbird*. Morphemes of type *a* (below) should be identified by their form and meaning.

Two types of morphemes, *a* and *b*, may be distinguished as follows: *a* may in most environments be freely substituted for by any one of an indefinite number of other single morphemes; *b* may in most environments be substituted for only by a limited number of single morphemes and in some frequently occurring environments hardly by any. In most sequences it is easy to tell *a* from *b*, e.g. *hat-s*, *teach-er* (*ab*), *old-er than* (*abb*), *the hat*, *to go* (*ba*). Morphemes which are not definitely *b* may be reckoned as *a*, e.g. *under*, *up*, *be*.

A sequence of morphemes is (part of) one word if: I. the sequence has the same distribution in relation to morphemes *b* as at least one single morpheme; or (and) II. (1) the sequence consists of morphemes *a* and *b*, and (2) it contains not more than one *a* unless the *a*'s together conform to I, and (3) the *a*, or cluster of *a*'s, may not be dropped without another *a* or cluster of *a*'s being substituted for it (unless so much may be dropped that the *b* of the sequence forms a normal sequence with another *a* in the environment), and (4) only such morphemes may be intercalated that the whole new sequence conforms to the other parts of II.

Sequences conforming to I only are compounds: *school-boy*, *over-turn*, Germ. *Tag-es-licht*, *aus-geb(-en)*. Some compounds may be "suspended": *ich gebe es aus*. Sequences conforming to I + II are derivatives; *a* is a root, *b* is a derivational morpheme. Type *ab(b): teach-er*, *nation-al-ize;* type *ba: un-clean*, *be-come*. Sequences conforming to II only are inflected forms, *b* is then an inflexional morpheme, henceforward symbolized as *c*. An example of *cac:* Germ. *zu-gĕb-en*, written as two words to distinguish it from *zŭ-geb-en* (*aac*), but cf. *ăus-zu-geb-en* (*acac*) written as one word; Eng. *to give* are two words because the *a* may be dropped (cf. II 3): *I want to*.

Examples of intercalation in *ac: hat-s*, *hatt-er-s* (*abc*), *hat-band-s* (*aac*, *aa* conforming to I); Fr. *pleur-er*, *pleur-nich-er* (*abc*); *donn-ons*, *donn-i-ons* (*acc*), *donn-er-i-ons* (*accc*); Lat. *grau-em*, *grau-ior-em* (*acc*). Morphemes *c* intercalated between *a* and *c* as above are semi-derivational because *a* + the intercalated *c* conform to I in relation to inflexional morphemes in the same word, but not in relation to further environments. Some morphemes may be partly *b*, partly *c:* Span. *niñ-a* (*ab*, distributed like *mujer*) versus *buen-a* (*ac*).

Special rules. 1. A sequence *ac* is one word in spite of intercalation of morphemes *(x)a'* whereby *a* becomes one word and *a'c* another word, if *(x)a'* are morphemes which might be dropped according to II (3), the parenthesis: *other-s*, *other boy-s;* Port. *clara-mente*, *clara e ràpida-mente*. Other special rules may be expected, e.g. for Eng. *there-of*, etc. (Acknowledgements: E. A. Nida: *Morphology*, 1949, particularly 4.6; A. Martinet in *Actes du VI^e congrès*, Paris 1949, p. 293.)

theme contains at least three consonants and may be analyzable into a root followed by another element (**leik^{w}* 'leave', **wel-p* 'hope'). The elements forming themes from roots should be called 'extensions' rather than 'suffixes', because their known distribution is hardly such that we may define them as genuine morphemes.[5]

Other nuclear morphemes and morpheme clusters will be called *stems*. They have usually more specialized functions than roots and themes, e.g. the presentic verbal stem **yu-n(e)-g-* 'yoke', the nominal stem **h$_1$ek'wo-* 'horse', the pronominal stem **k^{w}o-* 'who'.

3.1. It is possible that early PreIE had only one 'syllabic vocoid',[6] whose status of being or not being a vowel phoneme would depend on its distribution in words.[7] This conclusion may be drawn from three premisses: (1) The PIE vowels *i* and *u* (as well as *ṃ*, *ṇ*, *ṛ*, *ḷ*) are interchangeable with *y*, *w* (*m*, *n*, *r*, *l*), and seem to have had only consonantic functions originally; (2) PIE *ē*, *ō*, *ā* as well as *a* and the *o* which does not interchange with *e* are believed to have developed from *e* in the neighborhood of laryngeals. (3) The remaining PIE vowels *e*, *o* (and lengthened *ē*, *ō*) are interchangeable and seem to have developed from the same *e* as under (2). This so-called *fundamental vowel* will hereafter be written *ä*. However, the conclusion may be due to the over-simplification inherent in internal reconstruction; according to 1.3, we cannot know if two vowels, say, one high and one low vowel, had coalesced during the development from PreIE to PIE when new vowels, *i*, *u*, etc., were about to appear.

3.2. Many themes and stems have vowel-gradation so that an *ä* occurs after every consonant, though not simultaneously after all, e.g. *därk'ä—dräk'-s-* 'to see', *däywä—dyäw-* 'shine, god, Zeus'. From this it might be concluded that themes and stems originally had the syllabic pattern CVCVCV . . ., e.g. *däräk'ä*, *däyäwä*, and that one or more vowels had subsequently been syncopated in every form.

3.3. But this hypothesis is contradicted by another observation. It has been pointed out that there are heavy restrictions on the order in which consonants follow each other in PIE themes.[8] Themes do not usually end with two stops; in no themes is a medial stop or *s* flanked by two resonants (*y*, *w*, *m*, *n*, *r*, *l*), or even followed by a resonant; a medial laryngeal is not normally followed by *m*, *n*, *r*, *l*.[9] That is to say, the medial consonant of a PIE theme is not less open and

[5] Against E. Benveniste, *Origines de la formation des noms en i.-e.*, I, chap. IX; but otherwise the distinction between "root" and "theme" has been taken over, with minor changes, from Benveniste.

[6] For the meaning of this term see K. L. Pike, *Phonemics* (1947), pp. 4f.

[7] See 7.2 for a possible PreIE minimal pair *drāwā* 'run'—*dārwā* 'tree' which, if correctly assumed, indicates that the vowel was permutable with consonants.

[8] Karl Ammer, Studien zur idg. Wurzelstruktur (*Die Sprache*, II, Wien 1952, pp. 193ff., particularly pp. 200, 211–12; W. P. Lehmann, *Proto-Indo-European Phonology* (Austin 1952) 14.5. Both authors point out that the most open and sonorous consonants are found in the middle and the more closed ones in marginal positions, though they disagree about certain details.

[9] The alleged themes in *-ēi*, *-ēu*, etc. are probably spurious, so that *y*, *w* may be added to the list.

sonorous than the consonants on both sides of it; only *s* (and perhaps laryngeals) may occur initially and finally without regard to this rule. Such restrictions would be quite unmotivated if the syllabic pattern of themes had been CVCVCV . . .; they would be motivated if themes had originally been monosyllabic. When consonant sequences like *y-t-l* or *p-t-k* do not occur in themes, the reason for that must be that such sequences could not form monosyllables according to the syllabic patterns of PreIE. From this we may conclude that each PreIE root and theme contained only one fundamental vowel; the patterns were CVC and CVCC or CCVC and perhaps CCVCC. The contradiction between this conclusion and that of 3.2 must be solved.[10]

3.4. Neither in stems nor in inflected words do the consonants always pattern as one syllable, cp. *m-d'-w* 'mead', *p-k'-w* 'sheep', *s-w-p-n* 'sleep', *w-k^{w}-m* 'voice' (acc. sg.), *h*$_1$*-s-m* 'I am'. This fact may be interpreted in a diachronic sense: the more specialized types, i.e. stems and inflected forms, belong to a later period than the roots and themes. In early PreIE, the later roots and themes (or the vast majority of them) must have been monosyllabic words; in a subsequent period, the addition of morphemes sometimes made words polysyllabic. The variable occurrence of *ā* (3.2) may have started in the former period, e.g. as a variation between CVCC and CCVC ('thème I' and 'thème II' in Benveniste's terminology), but it seems to have been fully developed only in the latter period. The process must have been a redistribution of vowels and it must have taken place according to a certain rule; to find that rule is the aim of the following paragraphs.[11]

3.5. It is *a priori* probable that such a rule applied to complete word-forms, not to parts of words like themes or stems; similarly rules of syncope are normally operative within words, e.g. in ONorse or OIrish. The PreIE process may perhaps be viewed in the same light as the development of the Latin accent rules, i.e. as a means of strengthening the unity of words already definable as morphemic units (2.2.2). In that case the distribution of vowels in each word depended upon the length of the word, i.e. upon the number of consonants contained in it. In a language where the phonemic yield of vowels was very small (3.1), they might lend themselves to such a demarcative function.

The word-forms of this PreIE period may not have contained exactly the same morphemes as their descendants in PIE. Any PIE word-form which has either lost or added an ending after the PreIE period is misleading with regard

[10] K. Ammer, who accepts CVCVCV as the original pattern in themes, thinks that they became monosyllabic only after the vowel-reduction (*op. cit.* p. 211). It is difficult to see how. We should have to think that all themes whose sequences of consonants did not fit into the new monosyllabic pattern were either discarded or changed, e.g. by metathesis. At the time of the vowel-reduction, themes were probably often parts of words which did not become monosyllabic, and it is theoretically difficult to see why the themes should then be reduced to monosyllables at all costs.

[11] What follows is mainly a summary of two articles of mine in NTS XV (1949), pp. 137–87 and XVI (1952), pp. 136–47. The rule for the distribution of vowels is the same as in my earlier papers, but its starting-point has been modified here by the theory of originally monosyllabic themes.

to the rule of vocalization if the latter depended upon the length of words. It will therefore be the task of internal reconstruction to change certain PIE word-forms according to 1.2, if the distribution of vowels may thereby be reduced to a simple principle.

4.1. In some archaic athematic verbs the location of *ä* varies in accordance with the number of consonants so that at least the final part of each form presents clusters of two consonants. In the first, second, and third sg. of the present (imperfect) or aorist, the endings are the single consonants *-m*, *-s*, *-t* (the *-i* which occurs in the present only may be left out of this account); these forms are opposed to the third pl. with its biconsonantic ending *-nt;* the present participle follows the third pl. The first and second pl. and the dual forms are for various reasons difficult to interpret; cf. 7.1. The best examples are: *h₁äst—h₁säant* 'be', *h₁äyt—h₁yänt* 'go' (Ved. *éti, yánti*, cf. Gk. *eîsi*, part. *iónt-*), *gʷʿänt—gʷʿnänt* 'slay' (Ved. *hánti, ghnánti*, Hitt. *kwenzi, kunanzi*), *gʷämt—gʷmänt* 'come' (Ved. aor. *ágan, dgman*); verbs with infixed nasal: *hₓrnäwt—hₓrnwänt* 'move' (Ved. *ṛṇóti, ṛṇvánti*, Hitt. *arnuzzi, arnuwanzi*), *yunägt—yungänt* 'yoke', etc. The vowel occurs between the third and the second consonant from the end of each form.

4.2. Equally archaic is the relation between some thematic nouns, or nouns in **-e/os* or **-ā* (*-āh₂*) and, on the other hand, athematic nouns formed from the same theme or stem but with different location of *ä*. The forms are cited in the nom. sg.; the acc. sg. usually has the same structure, while other forms present special problems. *Däywäs* 'divine, god'—*dyäws* 'day, heaven, Zeus', *bʿälgäs* (Ved. *bhárgas* 'splendor')—*bʿlägs* (Gk. *phlóx*), *gʼʿäymäh₂* (Lith. *žiemà*, cf. Gk. *kheîma*) —*gʼʿyäms* (Lat. *hiems*, Gk. *khiốn*, Av. *zyå̊*), *pärkäs* (Gk. *pérkos* 'speckled')—*präks* (Gk. *próx* 'a young deer or roe'), *kärkäh₂* (Ir. *cerc* 'a hen')—*kräks* (Gk. *krex* 'a kind of bird').

4.3. The verb patterns are CVCC—CCVCC; the noun patterns are CVCCVC —CCVCC. The first noun pattern differs from the first verb pattern by containing V twice and by ending in a single C; also, the two noun patterns both contain four consonants but still have different vocalization. If we take away the final VC from the aberrant type, the relation between *däyw* and *dyäws* equals that between *h₁äst* and *h₁säant*. According to the principles of internal reconstruction, we may frame the following hypothesis: in PreIE, a class of uninflected nouns, e.g. *däyw*, were opposed to a class of nouns inflected by means of *-s* in the nom. and *-m* in the acc., e.g. *dyäws, dyäwm* > **dyēws *dyē(w)m*; at a later stage, some uninflected nouns received either case-endings (e.g. **deywos, *deywom*) or suffixes (e.g. **gʼʿeymoh₂*); the location of the stem vowel then remained unchanged.

But whence comes the 'thematic' vowel before the case-endings and the suffixes? The phonemic status of PreIE vowels renders it unlikely that the vowel alone was a suffix. Then these alternatives are left: (1) the vowel was inserted to avoid sequences of three consonants when endings or suffixes were added, e.g. *däyw+ s* > *däywäs*; (2) the redistribution of vowels after the period of monosyllabism had already introduced vowels in word final position, e.g. *h₁ästä—h₁säntä, däywä—dyäwsä;* these final vowels were normally dropped at a later

stage except if an ending or a suffix had been added in the meantime, as in **deywos*, **g'ʿeymoh₂*. The latter alternative will be preferred because it gives the simplest explanation of the forms mentioned in 4.4 and 4.5.[12] A final *-ā* should then be added to all the forms in 4.1, and the forms in 4.2 should be reconstructed as *dăywā—dyăwsā*, *bʿälgā—bʿlăgsā*, etc. There is now one rule for the vocalization of all these forms.

4.4. The uninflected type (C)CVCCV is represented by PIE neuters in **-u* and **-i* with *"flexion ouverte,"* i.e. gen. in **-we/os* and **-ye/os*. Their inflexion can be explained by the assumption that the gen. received an ending *-sā* before final vowels were dropped, while the nom.-acc. did not; some such nouns were at a much later stage transferred to the animate gender, e.g. Ved. *pašús* beside *pášu* and Goth. *faihu*, etc. Examples: *pášu*, *pašvás* < *päkʿwä*, *päkʿwā+sā*;[13] *mádhu*, *mádhvas*, Gk. *méthu*, *méthuos* < *mädʿwä*, *mädʿwā+sā*; Ved. *krátus* (m.), *krátvas* from *krătwä*, *krătwā+sā*; Ved. *náus*, *nāvás*, etc. from *näh₂wä*, *näh₂wā+sā*; Gk. *dóru*, *dourós*, and *gónu*, *gounós* < *dărwä*, *gʿänwä* (+*sā* in the gen.), cf. the derivatives Lith. *dervà*, OSl. *drĕvo* < *dărwā+h₂ā* or +*sā*; Ved. *dvis*, *dvyas*, Gk. *óis*, *oiós* from *h₃ăwyä*, *h₃ăwyā+sā*. Even stems in other consonants have sometimes received such treatment. From Gk. *kḗr*, Ved. *hā́rdi*, Lat. gen. *cordis*, etc. we may reconstruct a PIE paradigm **kʿ(h₁)ḗrd*, gen. **kʿ(h₁)r̥dés/ós* < *kʿ(h₁)ărdä*, *kʿ(h₁)ărdā +sā*. We may now add a fresh example to the list in 4.2: Hitt. *tekan* 'earth', gen. *taknaš* represents an uninflected stem *dʿägʿmä*, and the corresponding inflected nom. *dʿgʿämsā* is represented by Gk. *khthṓn*, Ved. *kṣā́s*, etc.[14]

Most uninflected nouns, however, received suffixes or endings which preserved the thematic vowel, e.g. Gk. *(w)érgon* < *wärgʿä+mä*, and the frequent type of masculines or feminines with *o*-grade, **gʿordʿos* 'enclosure', etc. The Hittite gen. in *-aš* of thematic nouns indicates that even in such nouns the gen. was originally formed in +*sā*, e.g. *wärgʿä+sā* like *mädʿwä+sā*; the resulting ending **-os* was superseded by **-osyo* or other endings in most dialects.[15]

4.5. There is a series of nouns comparable to that given in 4.2, but containing only two consonants in the stems. The vowel variations are then less violent: CVCV (+ endings) for the old uninflected forms, CVCCV for the old inflected forms. Examples: *pädä+mä* (Gk. *pédon*, etc.)—*pädsā*, acc. *pädmä* (Lat. *pēs*, *pedem*, etc.), *wākʷä+sā* (Gk. *(w)épos* n., etc.)—*wākʷsā* (Lat. *uōx*, etc.), *bʿärä+sā*, *+h₂ā* (Gk. *phóros*, *phorā́*)—*bʿärsā* (Gk. *phṓr*, Lat. *fūr*),[16] *gʷʿänä+sā* (Gk. *phónos*) —*gʷʿänsā* (Ved. *vr̥tra-há*, acc. *-hánam*).

4.6. The internal reconstruction performed so far on a number of archaic

[12] This final vowel is perhaps still seen in the secondary middle endings **-(n)tó*, etc., where it may have been preserved under the accent.

[13] The sign + is placed before endings or suffixes which were added after the rule for the distribution of vowels had ceased to operate.

[14] For a detailed discussion of these and related forms see NTS XVI, pp. 139ff.

[15] See H. Pedersen, *Etudes lituaniennes*, p. 23, and my paper in NTS VII, pp. 121ff.

[16] It is a necessary step in our internal reconstruction to assume that final *-s* in the nom. sg. has been dropped after resonants (which were themselves dropped in some dialects, Ved. *dātā* vs. Gk. *dṓtōr*); monosyllables have sometimes preserved the *-s*, e.g. Ved. *kṣā́s* vs. Gk. *khthṓn*.

IE word-forms, leads to the following *Rule for the Distribution of Vowels* (abbreviated RDV) which I tentatively assume to have been valid for a certain period of PreIE: *in words containing two consonants, both were followed by a vowel; the pattern was CVCV* (*pädä* 'step, ground'); *words with more than two consonants had a vowel after every uneven numbered consonant from the end, including the last one; the patterns were CVCCV* (*pädsä* 'foot' (*nom.*), *däywä* 'divine, god', *h₁ästä* 'is'), *CCVCCV* (*krätwä* 'power', *dyäwsä* 'Zeus' (*nom.*), *h₁säntä* 'are'). Examples of the pattern (C)CVCCVCCV will be found below, see notably 5.5 and 6.2. Addition of endings or suffixes after the RDV had ceased to be operative resulted in new patterns CVCV+CV (*pädä+mä*), CVCCV+CV (*däywä+sä*), etc.

4.7.1. The distribution of two different types of athematic verbs in the dialects is a point in favor of this hypothesis. One type presents a cluster of two consonants in all forms of the sg., in accordance with the hypothesis, e.g. **esm(i)*, **ess(i)*, **est(i)*. About 15 individual verbs have forms of this type which occur in more than one IE dialect (*h₁äsmä* 'am', *h₁äymä* 'go', *h₁ädmä* 'eat', *wäk'mä* 'wish' (Ved., Hitt.), *wästä* 'dresses', *gʷ'änmä* 'slay', *h₂wäh₁tä* 'blows' (Ved. *vāti*, Gk. *dēsi*); aorists: *gʷänmä* 'came' (Ved., Arm.), *d'äh₁mä* 'placed', *däh₃mä* 'gave', *stäh₂mä* 'stood up', *gʷäh₂mä* 'went', *päh₁tä* 'filled', *hₓärtä* 'rose', and a few more); in addition, there are the numerous optative forms of athematic verbs, e.g. Ved. *syāt*, Gk. *etē*, OLat. *sied* < *-yäh₁tä*. The other type, which presents a cluster of three consonants in the same forms, e.g. Ved. *dvéṣṭi* (**-yst-*), is not in accordance with the hypothesis. Such forms are found in Indo-Ir., Hitt., and OLith., but very few, if any, individual forms of this type occur in more than one dialect. We may conclude that the oldest stock of IE athematic verbs belong to the first type, while verbs of the second type were formed later. Cf. 6.3 about *s*-aorists.

4.7.2. There are two similar types of animate athematic nouns, one with a final cluster of two consonants in the nom. and acc. sg., e.g. **dyēws*, the other with a final cluster of three consonants, e.g. **nokʷts*. The distribution of these types is not so clearly in favor of the hypothesis. Numerous nouns of the first, favorable, type are attested in more than one dialect; beside those listed above we may mention *h₃räg'sä* 'king' (Indo-Ir., Ital., Kelt.), *h₂närsä* 'man' (Indo-Ir., Gk., Osc.), *k'wänsä* 'dog' (Indo-Ir., Gk., Lith. Kelt.), and the agent nouns in *-tärsä* > **-tē/ō(rs)*. But even nouns of the second type are attested in several dialects; beside the word for 'night', the most notable instances are all the stems in **-nt* (nom. **-nts*, acc. **-ntm̥*), e.g. the present participles. Judged by the RDV, these would be originally uninflected stems in *-äntä*, and the endings *-s* and *-m* would have been added after the final *-ä* had been dropped, as was also assumed for some stems in *-u* and *-i* (4.4). This explanation is confirmed if we may regard Goth. *menoþs* 'month < *moon' and perhaps Lith. *ménuo* (in spite of acc. *měnesį*, with which cf. Lat. *mensis*, etc.)[17] as an instance of the corresponding inflected type, viz. nom. *mäh₁nätsä* 'the measuring one'. OIr. *tene* 'fire' (**tepnētsʔ*), acc. *tenid*, may perhaps represent nom. *täpnätsä* 'the heating one'.

[17] H. Pedersen, *Etudes lituaniennes*, p. 51.

5.1. Many IE word-forms contain fewer fundamental vowels than stipulated by the RDV. That rule is exclusively based on the interchange -VC- — -CV-, which is evidently an archaic feature, and it does not cover the more common and productive interchange -VC- — -C-, as in Gk. *leípōn—lipón*. If the RDV is (approximately) correct, it must be assumed that certain vowels were subsequently lost.

That such a loss actually took place is indicated by doublets like **swepno-* (Skt., Lat. Gmc.)—**supno-* (Gk. Sl.) 'sleep', **k^{w}ek^{w}lo-* (Skt., Gmc.)—**k^{w}(e)k^{w}lo-* (> Gk. *kúklos*), **g'ʻeimā* (Lith., Sl.)—**g'ʻimā* (Skt.) 'winter, snow'. The fuller forms are evidently the older ones. That the cause of this interchange between *ā* and zero was a (stress) accent, is often concluded from cases like Gk. *leípōn—lipón*, *dṓtōr—dotḗr*, Ved. *émi—imás*, *dyáus—divás*. If so, the distribution of accents must have been changed in many cases (perhaps partly by simplification of paradigms with moveable accent), cf. Gk. *húpnos*, *kúklos*, Ved. *devás* (accented like *divás!*).

It cannot be assumed that only one vowel—the accented one—was preserved in each word-form; the thematic forms, the stems in **-e/os* and **-ā* and words like Gk. *dṓtōr*, Skt. *dātā*, would be hard to explain by that assumption. Maybe the facts are better accounted for by a rule of the following kind: if two vowels were preserved in one word-form, one of them had *o*-timbre (except under special prosódic conditions, as in vocatives and imperatives). The causes of the vowel-reduction and of the *o*-grade will not be further discussed here; it may only be remarked that analogical distribution of the various vowel-grades must have played a large part, above all in forms which have no fundamental vowel at all, e.g. **mn̥tis* 'mind', and probably in most forms with more than one *e*, e.g. **wek^{w}es-* (obl. stem) 'word', **b'eret(i)* 'carries'.

5.2. The following terminology may be useful: *old zero grade* = absence of vowel according to the RDV; *new zero grade* = absence of vowel due to the later reduction. The latter will sometimes be indicated by writing (*e*), e.g. **sw(e)pnos(e)* = **supnos*.

5.3. New zero grade occurs in the oblique cases of athematic nouns: **dyḗus—*diwés*, **diwéy; *k'wṓ(ns)—*k'unés*, **-éy; *g'ʻyṓ(ms)—*g'ʻimés* (Av. *zimō*). These oblique cases are formed in the same way as those described in 4.4 and are based on the old uninflected stem: *däywä+sä*, *+yä*, etc. > **d(e)ywés(e)*, **d(e)ywéy(e)*, etc. According to our hypothesis, they are of later origin than the inflected nom. and acc. But there are also older forms of the gen. and loc. in which the whole forms containing *-sä* and *-yä* are vocalized according to the RDV: Ved. gen. *dyós*, loc. *dyávi* = Lat. *Ioue* < *dyäwsä*, *dyäwyä*; cf. further the types *agnés*, *sūnós* (5.6.1), and some neuters like *dắru—drós*, *sắnu—snós*, which are evidently archaic. The Gmc. stems of the words for 'tree' and 'knee', viz. **trewa-*, **knewa-*, may then be accounted for as transfers to thematic inflexion based on the older type of oblique cases.

5.4. The opposition between uninflected and inflected nouns (*pādä—pādsä*, etc.) seems to have covered a semantic distinction roughly corresponding to inanimate versus animate. The uninflected forms may also have had adjectival

functions (e.g. *däywä* 'divine') which might explain their appearance in the new oblique cases of inflected nouns. The uninflected forms, being unmarked in relation to the inflected forms, probably had a less restricted sphere of employment than the latter.

5.5. Here is a list of derived stems with 4 or 5 consonants which occur in uninflected and inflected forms. In some of them a medial syllable has new zero grade. *Dh₃äträ* (Ved. *dátram* 'gift')—*däh₃tärsä* (Ved. *dâtā*, Gk. *dótōr*); Gk. *métron* 'measure' is formed like *dátram*, i.e. from *mh₁äträ*, while Ved. *mâtrā* and *dātrám* are later formations from the roots *mā*, *dā*. *G'änh₃ätlä* (> **g'enh₃(e)tlo-*, Lith. *žénklas* 'sign')—*g'näh₃tärsä* (Ved. *jñātâ* 'knower'; but Skr. *jñātram* 'intellectual faculty' must be a late derivative). *D'ämhₓäträ* (Skt. Lex. *dhamitram* 'an implement for kindling fire')—*d'mähₓtärsä* (RV. v. 9.5: *dhmâtā*). *G'änh₁ätyä* (> **g'enh₁(e)ty(e)*, Gk. *génesis*)—*g'näh₁täysä* (Ved. *jñātís* 'kinsman', Goth. *knodai* (dat.) 'tribe'). The meanings of these words suggest that *génesis* was originally uninflected (inanimate) as opposed to the inflected (animate) *jñātís;* however, *génesis* may also be a late derivative, since abstracts in *-ti-* usually have new zero grade in the stem, cf. Skr. *jātis* 'birth', which in any case represents the inanimate counterpart of *jñātís*. A very large number of derivatives in *-t-r* and *-t-y* do not conform to the RDV.

5.6. Animate nouns in *-i* and *-u* have two types of inflexion (apart from the originally inanimate "*flexion ouverte*," 4.4).

5.6.1. In the normal type, the final syllable has new zero grade in the nom. and acc., full grade in the gen. and dat., e.g. Ved. *agnís, -ím, -és, -áye*; *sūnús, -úm, -ós, -úve;* the same patterns are more or less clearly recognizable in most other dialects; in Gk. and Hitt. the endings *-os, -aš* in the gen. have been taken over from the "*flexion ouverte*," as has also the *-e* (< **-ei*) in the Vedic dative. Within the frame of our hypothesis, the ultimate source of this type cannot have been anything else than inflected nouns, e.g. nom. *h₁ägnäysä*, acc. *-äymä*, gen. *-äysä*, dat. (loc.) *-äyyä*.

5.6.2. In the other type, the pattern is reversed: the final syllable has full, usually lengthened grade in the nom. and acc., new zero grade in the gen. and dat., e.g. Ved. *sákhā, sákhāyam, sákhyuḥ, sákhye* = Av. *haxā*, dat. *haše;* Hitt. *ḫurtaiš, -ain*, gen. *-iyaš;*[18] Latin *uātēs*, gen. *-is;* Av. *-bāzāuš*, gen. *-bāzvō* 'having (strong) arms'. Evidently this type, too, represents old inflected nouns, but with a different distribution of stress (or of other prosodic features); if the normal type above was nom. **h₁égn(e)ys(e)*, gen. **h₁ognéys(e)*, the second type was nom. **sokʷhₓéys(e)*, gen. **sékʷhₓ(e)ys(e)*.

The Vedic inflexion of stems in *-t-r* follows the second type: *dātā, -tāram, -tus, -tre*, cf. Av. *dātā, -tārəm*, gen. *dāθrō;* in other dialects only the nouns of relationship preserve the new zero grade in the gen. and dat., e.g. Gk. *patḗr, -trós*. The Vedic gen. in *-tus*, < **-tṛs*, is a most interesting form, since it represents **déh₃t(e)-rs(e)* without addition of *-as*; we may conclude that *sákhyus* is a transformation, not of **sákhyas*, but of **sákhis* (= **sékʷhₓ(e)ys(e)* above); similarly Hitt. gen. *ḫurtiyaš* stands for **ḫurtiš*, and Av. *-bāzvō* for **-bāzuš*.

[18] E. H. Sturtevant, in *Mélanges H. Pedersen*, p. 57f.

5.7. According to 5.3 we distinguish between an old inflected type of gen. and loc. (*dyós*, *agnés*, *dātus*, **sákhis*) and a later type based on uninflected stems (*divás*, etc.). The paradigm Ved. *pánthās*, acc. *pánthām*, gen. *pathás*, Av. *pantå*, gen. *paϑō* now receives its explanation: nom., acc. **ponth₂s(e)*, *-m(e)*, gen. **pn(e)th₂é+s(e)*, i.e. a gen. of the later type. The genitives in **-tré/ós* (Av. *dāδrō*, Gk. *patrós*, etc.) and the datives in **-tréi* (Ved. *dātre*, etc.) are influenced by, or may directly represent, case-forms of this later type.

6.1. Thematic verbal forms are partly subjunctives and partly indicatives. The formal relation between the athematic ind. **h₁est(i)* and the subj. **h₁eset(i)* resembles that between the nouns **pēds* and **pedom* (4.5); it follows that thematic verbal forms represent uninflected words to which endings were added after the period of the RDV, e.g. *h₁ăsā*, *sărpă*, *+tā*, *+ntā*, etc. The modal sense of subjunctives being mostly rather vague, we need not hesitate to think that thematic subjunctives and indicatives have a common origin, viz. uninflected words functioning as predicates under certain syntactic and semantic conditions.[19]

6.2. Athematic verbs are often transferred to the more regular thematic inflexion; the thematic stem is then usually like the athematic stem of the 3. pl., e.g. Lat. *iungunt* = Ved. *yuñjánti*. In present stems with infixed nasal, the first syllable normally has new zero grade both in the original athematic and in the later thematic forms. But there are also, particularly in Gmc. and Lith., some thematic verbs of this class which contain a vowel in the stem located exactly where the RDV would lead us to expect it in the athematic 3. pl. Thus Goth. *rinnan* may be based on a 3. pl. *h₂rănwăntă*, cf. Ved. *ṛṇvánti;* Lith. *trénda* 'is eaten by moths' on *trăndăntă*, cf. Ved. *tṛṇdánti* (3. sg. *tṛṇátti* 'splits'); OHG. *springan* on *(s)präng'ăntă*, cf. without nasal Gk. *spérkhomai* 'hasten'. If this interpretation of *rinnan*, *trénda*, *springan*, and a few other forms is correct, they confirm that there is actually *new* zero grade in the first syllable of *ṛṇóti*, *ṛṇvánti*, and similar verbs, and that we may reconstruct, according to the RDV, *h₂ărnăwtă*, *h₂rănwăntă*, etc. But the evidence is somewhat uncertain since Gmc. and Balt. have largely systematized vowel-gradation; the full grade vowels in these verbs may have been analogically developed on the basis of inherited zero grade forms.[20]

6.3. Some Vedic themes with medial *r* show "metathesis" before suffixes beginning with a consonant, e.g. *s*-aorist *ádrāk(-ṣīt)* 'saw', fut. *drakṣyáti*, inf. *dráṣṭum* versus pf. *dadárša;* *s*-aor. *áprāk* 'mixed' versus root-aor. subj. *párcas;* *s*-aor. *ásrāk* 'threw' versus pf. *sasárja*, OIr. *selg* 'hunt'. With these we may compare Gk. aor. *érrexa*, fut. *rḗxō* (**wreg's-*; cf. pres. *rḗzō* from *wrăg'yă* + endings) versus pf. *éorga* and the noun *érgon* (the collateral forms pres. *érdō*, aor. *érxa* are

[19] That thematic indicatives and subjunctives have the same origin, is a conclusion drawn from Vedic usage by L. Renou, A propos du subjonctif védique (*BSL* 33, pp. 5ff.), p. 28.

[20] Cf. Brugmann, *Grundriss*₂ II, 3, p. 288f., where even some other possible examples of the same type may be found. For different etymological explanations (less likely, I think) of *springan* and *rinnan*, see *ibid.*, pp. 284 and 333.

probably analogical). These aorists conform to the RDV if they come from uninflected words containing a derivational suffix *-sä*, e.g. *drāk'sä*, *slāg'sä*, *wrāg'sä*. But in that case we expect thematic inflexion according to 6.1. The inflexion of *s*-aorists in Vedic, Greek, and Latin is not thematic, but neither is it the normal type of athematic inflexion. It shows no traces of either old or new zero grade in the plural active, as other athematic classes do. The personal endings have often unusual forms and are not the same in the three dialects. On the other hand, some *thematic sa*-aorists exist in Vedic, and in OSl. all *s*-aorists have half-thematic inflexion, e.g. 1. sg. *těchŭ* 'ran', 1. pl. *těchomŭ* from **tēkso-*, 2. pl. *těšte* from **tēks-te*. Even in OIr. the *s*-preterites are half-thematic; they are, however, mostly made from derived verbs and do not include any very old forms. But the same half-thematic inflexion reappears in the OIr. *s*-subjunctives which have stems of the same type as the OSl. aorists, e.g. **teks(e/o)-* (indic. *techid* 'flees', 1. sg. subj. *-tes*). From all this I conclude that the *s*-aorists were not originally athematic, inflected verbal forms of the type described in 4.1 and 4.7.1; they are rather based on uninflected words (*drāk'sä*, *tāksä*, etc.) which for some reason did not yield fully thematic paradigms when endings were added to them. The oldest paradigms may have been half-thematic, as in OSl. and OIr. In some dialects they seem to have been differentiated into athematic indicatives and thematic subjunctives, e.g. Ved. indic. *dvāṭ*, *dvākṣus*, subj. *vákṣat* (from *vah* 'carry', cf. OSl. *vĕsŭ*); Gk. aor. indic. *érrexa*, fut. *rézō*. The Vedic futures must then be derivatives in *-ye/o* dating from a period when the RDV was no longer operative.

Forms with clusters of three consonants, e.g. Gk. *édeixa*, Lat. *dīxī* (**-yk's-*) must be due to analogical levelling. May the Ionic form *édexa* stand for **ézexa* from *dyāk'sä*?[21]

6.4. Various PreIE suffixes, e.g. *-sä*, *-yä*, *-nä*, are found in both nominal and verbal forms in PIE; the reason for this seems to be that the suffixes formed uninflected words which could subsequently receive either nominal or verbal endings. Here are some examples showing the effects of such suffixes according to the RDV. *G^{w}'ärmä* (Ved. *gharmas*, Gk. *thérmos*)—*g^{w}'rämsä* (Ved. *ghráṃsas* 'heat of the sun', OIr. *grís* 'fire'), *$h_2$äwgä* (Ved. *ójas* from *$h_2$äwgä+sä*, Goth. *aukan*, etc.)—*$h_2$wägsä* (Gk. *a(w)éxō*, Goth. *wahs-jan*, Ved. pf. *vavákṣa*), *d'äwsä* (Goth. *dius* 'animal', OSl. *duchŭ* 'breath')—*d'wäsyä* (Lith. *dvesiù* 'breathe', *dvasià* 'spirit'), *pärk'ä* 'ask' (Lith. *peršù*, OHG. *fergōn*)—*prāk'nä* (Skr. *prašnas*, Goth. *fraihnan*); an inflected noun *prāk'sä*, *-mä* is seen in Lat. *prex*, *preces*; forms with analogical vocalization are e.g. Lat. *procus*, Goth. *fragan*. Examples of this type could be multiplied.

7.1. If we accept the material presented above as evidence for a "Rule for the Distribution of Vowels," we must conclude that this rule was operative at a very remote stage of PreIE. Its effects have very often been obscured by analogical levelling and by the new zero grade. Several morphological and derivational

[21] For other explanations of Ionic *dek-* see C. D. Buck, *Greek Dialects*, 49.1; E. Schwyzer, *Griechische Grammatik* I, p. 697, note 3.

forms must be later than the RDV; some of them are mentioned above, and here are some other instances.

Nearly all forms of the dual and plural of nouns belong here. The finals **-eyes*, **-ewes*, **-te/ōres* etc. in the nom. pl. are incompatible with the RDV; it is more difficult to judge the forms which have new zero grade, **-ins*, **-ibh/m-*, etc. However, the endings of the instr., dat., abl. pl. and du. vary considerably in the dialects, and Hittite has a defective and unstable system of case-forms in the plural; so everything indicates that these forms are fairly recent creations.

The derived nouns in **-r/n-*, **-i/n-*, and **-n-* yield very few examples of interchange according to the RDV; most of these derivatives seem to be later.

The 1. and 2. pl. and all the dual forms of athematic verbs also can be grouped here. These forms have often new zero grade (Ved. *imás*, *ithá*, etc.), but sometimes the evidence is conflicting, e.g. Ved. *smás*, OSl. *esmŭ* and Hitt. *ešweni*, Lat. *sumus*, not to mention Goth. *sijum*. I can find no basis for a reconstruction of these forms according to the RDV.

The perfect, with which the Hittite *ḫi*-conjugation must be connected, is another difficult category. I assume, rather vaguely, that it is based on some kind of uninflected words (like the *s*-aorist), and that the inflexion is post-RDV.

7.2. The RDV may possibly have suffered certain original exceptions. In many themes an initial *s* is never followed by a vowel, so that *st-*, *sp-*, etc. seem to have functioned as single phonemes: we may reconstruct a theme *stäh₂ā* 'stand', but there is no evidence for a form *säth₂ā*.[22] Maybe this has some connexion with the problem of *s mobile*, cf. OIr. *-tá* 'is'. Several themes with a medial and a final resonant have the pattern CCVCV instead of CVCCV. They have thematic forms like *k'läwä* 'hear' (in **k'lewos* 'fame'), *sräwä* 'flow', *pläwä* 'float, rain', *dräwä* and *drämä* 'run', *k'läyä* 'lean', *trämä* 'be afraid', whereas the pattern *k'älwä* is not attested. But as nominal stems, we find *g'änwä* 'knee' and *därwä* 'tree' (distinguished from *dräwä* 'run' only by the location of the first vowel?). According to 3.4, these stems (like *päk'wä* 'sheep') may have been formed after the period of monosyllabism, while most themes would go back to that period; thus we may suspect that the themes have retained a vocalization which was once required in monosyllables, i.e. *k'läw*, *k'läy*, not *k'älw*, *k'äly*, etc. There are, however, some themes like *k'läpä* 'steal', *träpä* 'turn away', to which this explanation is hardly applicable; perhaps their unchangeable vocalization is the result of later levelling based on derivatives like *k'läpsä* (Lat. *clepsī*, Gk. *éklepsa*).

University of Oslo

[22] The aspirate of Skt. *sthā* may be the result of contact between *t* and the laryngeal after the development of new zero grade in **st(e)h₂e*.

LE *FRANZÖSISCHES ETYMOLOGISCHES WÖRTERBUCH:* ÉVOLUTION ET PROBLÈMES ACTUELS

WALTHER VON WARTBURG

Celui qui a entrepris, dans ses jeunes années, une œuvre scientifique d'une certaine ampleur et s'y voue pendant toute sa vie, ne manque pas de s'apercevoir avec le temps qu'elle se développe selon ses lois propres et conformément à sa nature. Telle a été la destinée du dictionnaire étymologique du français, FEW (*Französisches Etymologisches Wörterbuch*). Si l'on jette un coup d'œil sur les volumes qui s'échelonnent entre les premières livraisons et les derniers fascicules, on s'en rend immédiatement compte. Si je ne me sentais pas toujours plus pressé de rédiger la plus grande partie possible de ce qui me reste à écrire pour achever mon œuvre, je serais tenté d'esquisser une histoire de cette entreprise et d'entrer à ce propos dans quelques détails. Je ne puis indiquer ici que quelques étapes de cette évolution.

Une première grande innovation, introduisant une coupure nette dans l'ouvrage, a été de tenir, à partir du milieu de la lettre B, un large compte de l'évolution de la langue écrite. Ayant été, comme tous ceux de ma génération, orienté par mes études vers le vieux français, la dialectologie et la géographie linguistique, je n'ai été amené que par des expériences personnelles à reconnaître l'importance majeure de l'évolution du vocabulaire de la langue écrite et du français moderne pour l'ensemble du galloroman. Ce n'est qu'après la publication d'une grande partie du premier volume que je me rendis clairement compte combien cette évolution était peu connue, bien moins que celle des dialectes. Je m'avisai seulement à partir du moment où je m'enhardis à remonter aux sources de la langue écrite que les problèmes se rapportant aux dialectes se confondaient souvent avec ceux de la langue écrite. A cette époque, je présentai, au premier congrès des Romanistes à Dijon, un court aperçu des expériences que je venais de faire.[1]

La réalisation systématique de ce genre de recherches pour les besoins de l'FEW ne fut possible que lorsque j'eus été appelé à Leipzig. A Leipzig, je pus songer à former des collaborateurs et à m'assurer de leur aide. Ce n'est qu'alors qu'il fut possible d'atteindre ce but désiré: une utilisation plus ou moins complète des sources de la langue écrite. Toute une méthode fut créée pour l'exploitation de ces sources surtout par le mérite de deux jeunes romanistes, Werner Hering et Erich Poppe.[2]

Les premières années de la période où l'FEW eut son siège à Leipzig, une nouvelle tendance dans le traitement des problèmes s'introduisit dont l'importance s'est encore peu manifestée jusqu'ici. Dès le début, une condition

[1] Cette conférence a été publiée en allemand: *Behrens-Festschrift*, Jena–Leipzig 1929.

[2] Tous deux sont malheureusement perdus aujourd'hui pour l'FEW, Hering, qui est mort à la guerre, et Poppe, dont la réintégration dans le groupe des collaborateurs de l'FEW à la fin de la guerre s'est heurtée à des difficultés matérielles et qui s'est consacré par la suite à d'autres tâches.

essentielle de ce travail avait été de résoudre les questions relatives au groupement des mots, à leur succession, à la façon dont ils sont remplacés les uns par les autres, à leur répartition géographique et à leur déplacement d'une région à l'autre. En cela, l'FEW suivait, d'une part, la tradition de l'onomatologie, qui avait été développée et mise au point peu avant la première guerre mondiale dans divers travaux parmi lesquels je peux citer ma dissertation inaugurale et la critique pénétrante qu'en a donnée K. Jaberg. D'autre part, des préoccupations analogues avaient été provoquées par la géographie linguistique telle que la concevaient Gilliéron, Jud et Jaberg. Cette façon de mettre en valeur l'interdépendance des mots dans une certaine sphère sémantique trouva alors son épanouissement dans la conception, issue de nos travaux, du vocabulaire formant un tout et exprimant les divers aspects d'une représentation du monde telle qu'elle se produit chez l'homme moyen. Il est, au reste, évident que cette représentation n'est pas exempte de lacunes et qu'elle est sujette à des variations dépendant du degré de culture et de la position sociale de l'individu. Cet élargissement de mes vues premières a été provoqué en grande partie par la lecture de l'œuvre de Humboldt. En suite de cet agrandissement de notre perspective, le programme de la seconde partie de l'FEW[3] fut complètement modifié. Non seulement chacun des concepts isolés devait être mis en contact avec l'ensemble, mais le but du dictionnaire devenait la description, d'une part, des transformations du monde conceptuel en tant qu'elles se manifestent par une modification du vocabulaire et, d'autre part, des changements de mots provenant de causes purement linguistiques. Entreprendre une classification des concepts en tant qu'ils s'expriment par des mots, et faire en cela une œuvre qui puisse servir pour longtemps de base de recherches, tel fut alors le désir le plus ardent des romanistes qui se rattachaient d'une manière ou d'une autre à l'FEW. L'un d'entre eux, Rudolf Hallig, tenta cette entreprise en restant toujours en contact avec moi. Son ouvrage fut imprimé sous une première forme à maints égards imparfaite et sous le titre de Questionnaire. Il devait servir de base, d'une manière générale, pour les enquêtes linguistiques et a été utilisé effectivement lors des enquêtes qui furent faites pour l'élaboration de l'*Atlas Linguistique de la Lozère*. Il a été imprimé depuis sous une forme différente et sous ce titre: Rudolf Hallig und Walther von Wartburg, *Begriffssystem als Grundlage für die Lexikographie*, Versuch eines Ordnungsschemas, Abhandlungen der deutschen Akademie der Wissenschaften zu Berlin, Klasse für Sprachen, Literatur und Kunst, Jahrgang 1952 Nr. 4.

Le transfert du siège de l'FEW à Bâle entraîna, après un arrêt dû à une réorganisation devenue nécessaire, une nouvelle orientation de notre travail. En Suisse, nous eûmes le privilège d'être secondé par de jeunes romanistes dont le français était la langue maternelle. Cet état de chose eut pour conséquence un enrichissement de notre œuvre. La participation directe des collaborateurs à notre recherche les incita à mettre à profit leurs lectures personnelles. Et depuis que le dictionnaire est rédigé à Bâle, ils ont apporté un genre de contribution qui faisait entièrement défaut auparavant: les témoignages personnels au sujet de

[3] Cf. FEW vol. I. p. iv sqq.

l'usage des mots. Cet apport a gagné en importance depuis 1946. Dès cette époque, en effet, l'FEW s'est assuré aussi la présence permanente de collaborateurs venant de France.

L'installation à Bâle a causé encore un changement d'un autre ordre. Dans la préface à la seconde partie du second volume, j'avais fait allusion aux grandes difficultés que nous avions à continuer la publication de l'ouvrage. Ce n'est que cinq années après mon retour en Suisse que le premier fascicule du volume II, 2 put paraître. Ce retard, qui semblait tout d'abord menaçant pour l'avenir de l'ouvrage, devait avoir aussi des conséquences heureuses. Pendant toute la période où la publication fut arrêtée, la rédaction ne cessa d'être poursuivie. Jusqu'alors le programme de la rédaction avait dépendu du rythme de l'impression. Il devait y avoir dans l'ordre alphabétique une quantité toujours suffisante de manuscrit pour assurer un débit régulier. Dès le moment ou cette contrainte cessa, il fut possible d'ordonner le travail de rédaction au gré des rapports que les mots ont entre eux. Tandis que l'article APPLICARE avait été rédigé isolément, IMPLICARE ne fut pas traité sans que PLICARE fût étudié en même temps. Dans cette confrontation, des constatations purent être faites pour PLICARE qui étaient valables aussi pour l'histoire d'IMPLICARE, sans parler d'APPLICARE dont l'article, à la lumière de l'article PLICARE, paraît maintenant insuffisant à plusieurs points de vue. De même, l'article *INTORTILIARE ne pouvait être écrit sans l'étude de TORQUERE. Or on ne peut délimiter la zone de TORQUERE par rapport à TORNARE, avec lequel il se confond phonétiquement sous certaines formes, que lorsque TORNARE est lui-même rédigé. Pour des raisons d'ordre sémantique, cette étude exige en outre un examen du groupe des verbes qui proviennent de VOLVERE et de VERTERE et de ceux qui correspondent au français *virer*. Grâce aux articles qui se sont accumulés au cours de ces années, il nous est permis aujourd'hui de nous libérer de la contrainte de l'alphabet et de nous laisser conduire par les affinités des mots sans devoir interrompre pour autant l'impression de l'ouvrage.

Lorsque je reçus de New-York l'invitation à fournir une étude pour *Linguistics Today*, l'idée me vint d'exposer ce dernier aspect sous lequel je conçois la constitution de l'histoire des mots. Je portai mon choix sur un groupe de mots que j'avais été obligé de traiter au cours d'une étude du mot *étuve*, entreprise de la manière que je viens de décrire. Le point de départ avait été la rédaction des articles *IMPELTARE et *IMPUTARE, deux verbes qui ont le sens de «greffer». Ayant reconnu que ces deux vocables avaient une origine grecque, je fus amené à déterminer dans quelles proportions l'influence du grec de Marseille s'est fait sentir sur le vocabulaire galloroman (autant du moins que mes études me le permettaient[4]). Au cours de cette recherche, je rencontrai le mot *étuve*, en marge duquel une série d'autres mots surgirent. L'hospitalité des éditeurs du *Word* me fournit l'occasion, bien avant que ces articles étroitement liés les uns aux autres paraissent dans l'FEW[5], d'en publier au moins le commentaire, accompagné d'un choix très limité du materiel qui y est discuté. Dans l'FEW, quel-

[4] Cf. *ZRPh* 68, 1–48 (article dont a paru un tirage à part).

[5] A cause de l'espace limité dont nous disposons la masse des matériaux ne peut être indiquée que de manière abrégée.

ques-uns de ces articles figureront sous S, d'autres sous T et d'autres encore dans le volume réservé aux mots d'origine germanique. Cela signifie que, entre la publication du premier et celle du dernier, dix années se seront écoulées, sinon plus, si bien que le lien qui les a unis lors de leur élaboration n'apparaîtra plus, et que le lecteur, en dépit de son attention, remarquerait à peine cette union. Or, grâce à l'initiative des éditeurs du *Word*, l'interdépendance de ces articles sera mise en évidence. Cette publication a encore un autre avantage. Le lecteur constatera que je n'ai pas réussi à faire partout la lumière et que plusieurs points prêtent à des discussions. Il serait très heureux que les linguistes voulussent bien intervenir; par leurs suggestions, ces articles pourraient avoir dans l'FEW une forme plus définitive que ce n'est le cas aujourd'hui.

Les articles ne réclament guère un commentaire plus ample que celui qui a été donné, lors de leur rédaction, à propos des étymologies. Je désire cependant montrer encore comment ils se sont groupés au moment où je les ai rédigés. *Etuwe* se rattache à la famille du grec *týphein*. *Etuwée*, qui en a été dérivé, exigeait, par suite de son affinité sémantique avec *étouffée* et *étouffade*, un examen d'*étouffer*. Etant parvenu à la conclusion qu'*étouffer* provient d'un croisement entre *estofer* et *estoper*, je dus établir l'histoire du francique STOPFON et celle du latin STŬPPA. Ce dernier mot à son tour, était si voisin de *toupe* 'touffe', pour le sens et pour la forme, qu'il n'était possible de lui attribuer à coup sûr tous les dérivés (*toupillon—estoupillon*, *toupette—étoupette*, etc.) qu'après avoir fait un inventaire complet des deux familles de mots. C'est ainsi que j'en vins à rédiger l'article sur l'ancien francique TOP, pour lequel il fallut encore examiner l'origine de *toupie*. Comme *toupie* a été mis en rapport à plusieurs reprises avec les mots galloromans du type *toupin* 'pot', l'article *TOPPĪN s'imposait. D'autre part, comme l'a montré Jud, *Revue de linguistique romane* 1,194, STŬPPA a eu sur la famille de mots galloromane du type *estuper* une action telle que la voyelle du radical en a été modifiée; d'où l'article *TUPP- 'sombre'.[6] Par le fait que beaucoup de représentants plus récents du vieux français *estoper* ont perdu leur préfixe, il était nécessaire de délimiter le mot *toper*, d'où l'article TOPP-. A cela s'ajoute un petit problème secondaire: le rapport entre le moyen français *toupebras* et le français moderne *toupras*, d'où l'article: néerlandais TOPPEN. Malgré leur ressemblance phonétique et leur signification analogue, les verbes *toper* et *tauper* doivent être séparés l'un de l'autre. *Tauper* provient d'un *TALPA 'patte' dont l'origine reste obscure. Pour en faire la démarcation d'avec TALPA 'taupe', il n'est pas nécessaire de publier ici cet article parce qu'il ne fournit pas beaucoup plus de renseignements que ceux qui nous sont donnés par Bertoldi, *Bulletin de la Société de Linguistique de Paris*, 32,150, par Schürr, *Zeitschrift für romanische Philologie*, 47,492 et par Jud, *Festschrift Gauchat*, 309. L'étude du verbe *étouffer*, d'autre part, a entraîné celle de TŪFA.*

[6] Un autre mot *estuper* provient du moyen hollandais *stupen*. Voir Roques, *Romania* 41, 608.

* On a laissé aux notes des différents articles les numéros qu'elles portent dans le texte original et complet. On reproduit ici seulement les notes qui se rapportent aux matériaux publiés ici même.

TYPHEIN (grec) 'fumer'

I.1.a.—Apr. *tubar* v.n. 'fumer' (Briançon XV^e^ s.), Isère *tuba*, hdauph. *tubà*, Lallé *tubar*, pr. *tubà*, aveyr. 'faire du brouillard' . . .—Dér. Pr. Puiss. *tubado* f. 'fumée'. Périg. *tubeià* v.n. 'exhaler'.

b. Mdauph. bdauph. pr. castr. aveyr. *estubà* v.a. 'fumiger'.—Dér. Pr. aveyr. *estubado* f. 'fumigation'.

2. Fr. *estuve* f. 'salle de bains, surtout pour bain de vapeur' (Rs-Pom 1700), apr. *estuba* (rouerg. vers 1220; Bonis), *stuba* (Millau 1520, Doc. 464), mars. Toulouse *estubo*, béarn. *estube*; mfr. frm. *estuve* 'lieu clos dont on échauffe la température pour transpirer' (Paré-Trév 1732), *étuve* (dp. Mon 1636) . . .—Dér. Afr. *estuver* v.n. 'prendre un bain de vapeur' (hap. XIII^e^ s.), fr. v.r. (RoseM–Pom 1700), *étuver* 'laver, bassiner (une plaie)' (dp. 1534); *estuver* 'cuire à l'étouffée' (1393), Malm. verv. *stufé*, liég. *stoûver*, LLouv. Mons *stuver*, Jers. *étuiver;* pr. *estubà* 'bonifier un tonneau en y mettant des raisins bouillis'; Lyon *étuó* 'mettre de l'eau pour faire gonfler le bois', louh. *étiver*. Afr. *estuee* f. 'chaleur' (vers 1180), aveyr. *estubado* 'lieu plein de vapeur chaude'; mfr. frm. *estuvee* 'manière de cuire les aliments en vase clos, par la vapeur d'eau' (XV^e^. s-Trév 1732), frm. *étuvée* (dp. Mon 1636), 'viande assaisonnée et cuite en vase clos' (dp. Ac 1694), liég. *stoûvèye* 'ce qui a été cuit à l'étuvée', verv. *stoûfêye*, Jers. *étuivée*. . . .

II. 1. Vsoan. *htüa* f. 'poêle pour chauffer la chambre', Noasca *stüa* . . . AIS 938.

2. Aost. *estouf* adj., 'qui est dégoûté d'un mets'; *estouflé* v. 'donner du dégoût'.

3. a. Frm. *estoufade* f. 'façon de faire cuire les viandes et les légumes en vase clos' (Wid 1669-Boiste 1803), *estouffade* (dp. Ac 1835), *étouffade* (dp. 1658, Scarr).

b. Frm. *étouffée* f. 'façon de faire cuire les viandes et les légumes en vase clos' (dp. 1864), Provins *étoufouée*, St-Nab. *eitoffoudie*.—Alais *estoufà* m. 'étuvée de viande cuite sans eau, à la braise', Puiss. Tarn, aveyr. Agen *estoufat* 'bœuf cuit à l'étuvée'.

Le grec *týphein* 'fumer' a passé de Marseille dans le langage du sud de la Gaule. Le *-b-* de l'ensemble des formes occitanes remonte à une prononciation *-p-*, et montre donc qu'à l'époque de l'emprunt le *-ph-* était encore une aspirée et non une spirante. Des mots empruntés plus tard, comme *néphos* 'nuage'. ont un *-f-* en occitan. Le passage de *týphein* dans la langue des environs de Massalia est probablement en relation, comme c'est le cas pour *pŷr* 'feu', avec le système de signalisation que les Grecs avaient organisé le long de leurs voies de navigation. Les colonnes de fumée qui s'élevaient vers le ciel jouaient, le jour, le même rôle que les feux, la nuit. On ne risque pas de se tromper en supposant que les Grecs employaient des indigènes pour ce service et que, par ce moyen, le verbe s'est introduit dans leur langue. Certainement, ce mot a passé dans les dialectes gaulois des côtes de la Méditerranée et de la vallée du Rhône avant la conquête du pays par les Romains (128 av.J.Ch.) et il s'est maintenu sous la forme **tūpare* lorsque, peu à peu, le gaulois a été évincé par le latin. Parmi les formes galloromanes, celles de I ont évolué selon les lois phonétiques locales. 1 se rapporte au

sens de 'fumer'; il est fortement représenté en occitan, en partie dans le sens de 'brouillard'. Il vit surtout dans le Dauphiné et la Provence et s'étend vers l'ouest jusque dans le Rouergue, le sud de l'Auvergne et le Périgord, régions où il est moins fréquent. Le Dauphiné et la Provence, principalement, ont encore un dérivé formé avec le préfixe *ex-* (a). Son apparition isolée dans la région de Castres et dans le Rouergue montre qu'il s'étendait jadis vers le sud aussi loin que le simple (b). Alors que le simple se cantonne dans le sens de 'dégager de la fumée', les représentants du type **extupare* signifient 'fumer (le lard)'. Le même verbe a été déjà très anciennement employé pour désigner la technique qui se rattache aux bains chauds. Il s'agissait vraisemblablement de bains qui se prenaient dans une salle remplie de vapeur d'eau. Dans ce sens, **extupare* a gagné, du sud, le nord de la Gaule. Le verbe est attesté en ancien français plus tardivement et moins souvent que le substantif *estuve* qui en a été dérivé pour désigner le local des bains. Celui-ci se trouve déjà sous la forme *stupa* dans les gloses de Kassel. Le rapport entre les deux formes doit être en conséquence le suivant: dans le sud de la Gaule, un substantif **extūpa* a été formé à partir de **extūpare* pour désigner le local où se prennent des bains de vapeur; le substantif seul, sans le verbe, a été exporté dans le nord, et ce n'est qu'en ancien français qu'un verbe *estuver* a été reformé sur *estuve*. De là: 2.[11] Le mot, avec le sens du français *étuve*, paraît être retourné du nord dans le sud de la France. C'est ce qui ressort de la chronologie des textes et du fait que la signification de 'local chauffé' (p. ex. Marseille) se trouve entièrement isolée en face de la famille bien déterminée de 1a. Le catalan *estuba* 'bain de vapeur' est emprunté au galloroman; le moyen breton *styff* 'lavoir', le breton *stiv* 'cloison de navire', *stivel* 'fontaine', etc. sont empruntés au mot français, qui survit aussi dans l'anglais *stew*. L'histoire du toscan *stufa* 'fourneau', de l'émil. *štǫeva, štūa*, du lomb. *stūa*, du piém. *stūwa, stūvia*, du vén. *stva*, du frioul. *štǿvç*, du tess. *stūva* 'chambre chauffable', du sursilvain *štiva*, de l'engad. *štūgva* (AIS 938, 939) est analogue à celle du français *étuve*. Ce substantif est, de même, attesté beaucoup plus anciennement que le verbe *stufare* 'chauffer, faire cuire à l'étuvée' qui est d'ailleurs moins usité dans la première signification que dans la seconde. Il remonte à un type **extūfa* qui, pour sa part, doit être dérivé d'un verbe **extūfare*. Celui-ci a été formé de *ex-* et de l'emprunt grec *týphein* à une époque plus tardive qu'**extūpare* en Gaule. Cela s'explique par le fait qu'il a été emprunté à l'Italie méridionale à une époque où *ph* avait déjà passé à *f*.[14] L'ancien lombard *stuwa* 'bain chaud' (*Archivio glottologico italiano* 12,436) et des dérivés comme l'anc.vén. *stuovèr* 'baigneur'

[11] Nous laissons de côté la question de savoir si l'allem. *stube* (équivalent du français *poêle*), qui désignait originairement le seul local chauffable de la maison, remonte au roman. Il est en relation avec le néerl. *stoof* 'four de séchage', anglosaxon *stofa* 'bain', anglais *stove* 'fourneau', anc.nor. *stofa* 'salle de bain' que Franck W. croit être d'origine germanique.

[14] De même, le subst. *tŷphos* 'fumée', qui est la base du verbe, est encore très vivant dans les langues du sud de la Romania. Par ex.: vén. *tufo* 'vapeur', Gröden *tof* 'odeur', Fassa *tǫf* 'chaleur étouffante', véron. *tuin*, piém. *tūf, tufor*, Val Sésia, Alexandrie *tuff*, piém. *stufè* 'suffoquer de chaleur', Lucques *tufa* 'chaleur étouffante' AGl 12, 134, Chiana *tufea* 'brouillard épais', Sard. *tufu* 'tanfo' ARom 24,56, cat. *tuf* 'vapeur', *estufar* 'boursoufler', *atufarse* 'étouffer', andal. 'abochornarse', esp. *tufo* 'vapeur; mauvaise odeur', Salamanque

(Mussafia, A., *Beitrag zur Kunde der norditalienischen Mundarten im 15. Jahrhundert;* Wien 1873), le latin médiéval *stuphare* 'fréquenter l'étuve' (Nigra, C., *Saggio lessicale di basso latino curiale compilato su estratti di statuti medievali piemontesi;* Torino 1920) montrent que l'évolution du sens a passé par les mêmes phases. L'espagnol *estofar* 'faire cuire à l'étuvée, rôtir' et *estovar* sont empruntés à l'Italie méridionale. De même le portuguais *estufa* 'fourneau' et le catalan *estufa* 'bain de vapeur' viennent de l'italien *stufa*. De la signification 'faire cuire à l'étuvée' est issue celle de 'rassasier qq. de qch.', d'où l'adj. italien *stufo* 'rassasié, écoeuré'. Ce groupe italien a été emprunté par le français en ce qui concerne quelques-uns de ses éléments (II). Le piém. *stüwa* a pénétré dans les vallées alpines franco-provençales et provençales (1). Il en est de même de *stufo* (2). La forme particulière de l'aost. provient du verbe piém. *stofiè* 'répugner à qq.', avec transformation de *-fi-* en *-fl-* dans la haute vallée. 3 est emprunté à l'italien *stufata* 'rôti, daube'. Il a été tout d'abord employé sous la forme de l'it. sept., puis, par le redoublement du *f*, il a été assimilé au français *étouffer* (a) et il a été enfin francisé aussi dans sa terminaison (b). Voir, plus haut, *étuvée* qui est identique morphologiquement et a la même signification.

Bugge R 4,354; Schuchardt Zdwf 1,66; Meringer IF 18,273; Meyer-L., Prager Deutsche Studien 5,78; Kr Jber 11,110; Zdphil 27,52; Gay; Hehn 143; Schrader 2,122; WS 9,1; Kretschmer 505; Gerig 39; Jud BDR 3,6; Arch 124,398; Brüch 5; Arch 145,103; 164,148.

STOPFON (francique) 'introduire de force, remplir complètement'

I. Afr. *estoffer* v.a. 'rembourrer (un meuble, un collier, etc.)' (vers 1260), mfr. id. (1388, Hav; hap. XV^e^s., Lac; Oud 1660), *étouffer* (1530, Palsgr 741b); anc.fr. *soi estoffer* 'se munir de ce qui est nécessaire pour un but déterminé' (vers 1190), anc.fr. moy.fr. *estofer* 'munir, garnir de tout ce qui est nécessaire, équiper, approvisionner' (1224–1318), *estoffer* (XIV^e^ s.—Voult 1613, Gdf; DG; Li; Runk; Chastell; Haust Rég 2; JLemaire; Gaillon 378,386; Malherbe; Héroet), *estoffer l'estat de* 'fournir à la dépense de' Froiss; anc.liég. *estoffer* des frais 'compenser' (1378, Haust Rég 1), *stoffeir* JStav; mfr. frm. *estoffer* 'orner, embellir (p.ex. les armes, les sculptures par des applications d'or)' (1350-Trév 1732, Hav; Li; Lac; Hu; Scarr), *estouffer* (1557, Lac), *étoffer* (1636—Trév 1771, Ord 16,184); *estoffer* 'garnir de tout ce qui est nécessaire pour augmenter la commodité de qch., pour donner de l'ampleur à qch.' (1385—Trév 1732), *étoffer* (dp. Ac 1740); *estoffer Dieu* 'donner à Dieu la matière nécessaire pour l'incarnation en consacrant l'hostie' D'Aubigné; *estoffer* 'créer qch, en fournir la matière' Montaigne; 'rendre plus riche en faits (p.ex. un récit, un discours)' Montaigne, *étoffer* (1700, Bayle;

tufar 'puer', Murcia *tufada* 'puanteur'. Voir aussi ML 9024. Brüch (Arch 145,103; ZFSL 52,404) pense qu'**extufare*, originaire d'Italie, remonterait à un ancien **extupare* qui aurait échangé son *-p-* contre *-f-* sous l'influence de *tufo* provenant du grec *tŷphos*. Dans ce cas, il serait étonnant qu'il ne fût resté nulle part une trace de **extupare*. Une meilleure explication de l'état de choses à l'époque romane est fournie par une ancienne formation **extupare* dont le point de départ est Marseille où le simple *týphein* a été, lui aussi, prononcé avec *-p-*, et par un plus récent **extufare* qui est issu du grec de l'Italie méridionale et qui est en concordance avec le résultat de *tŷphos* en Italie.

dp. Lar 1930); *estoffer* (un habit) 'employer l'étoffe nécessaire pour' (Cotgr 1611; Ac 1694–1718), *étoffer* (dp. Ac 1740); *étoffer* de la crème 'donner du volume en remuant continuellement' (Enc 1756–DG); *estoffer les maschoires* 'faire un bon repas' (1610, Sainéan Par).

Dér.—Fr. *estoffe* f. 'matière, toute sorte de matériaux (p.ex. pour construction)' (1241–Trév 1732), *atoffe* (lorr. 1354), *stoffe* (anc.liég., JStav, BTDial 18,371; 1441, Haust Rég 3), *atouff* (frcomt. 1485, Mém Jura 1901, 286), frm. *étoffe* (Mon 1636—Ac 1878), Chablis *étoffes* 'merrain, cercle, osier, tout ce qu'il faut pour faire une feuillette', Lunév. *de l'atoffe* 'de la matière, de l'étoffe'; mfr. *estoffe* 'équipement' (Froiss; vers 1410; Chastell), apr. *estoffa* (hap. XV^e^ s.); argot *estoffe* 'part du vol, butin' (1455), mfr. 'argent' Chastell. Afr. *estofes* 'fournitures (pour brodeuses)' Maillart, mfr. *estofe* 'ingrédients' (St-Amand XIV^e^ s.), frm. *estoffes* pl. 'soie retorse entortillée sur la broche des brodeurs' (Rich 1680–Trév 1771). Mfr.frm. *estoffe* f. 'tissu de soie, de laine, de coton, etc., dont on fait des habits, dont on recouvre des meubles, etc.' (1599–Trév 1732, Hav), apr. *estoffo* (Carc. 1442), frm. *étoffe* (dp.Mon 1636).

II. 1. Châten. *chtoufaî* 'oppresser (l'estomac)'.

2. Plancher *chtoppa* 'bourrer'.

Cette famille de mots s'est généralisée en Europe sous la forme de substantifs: it. *stoffa*, cat. esp. port. *estofa*, allem. *stoff*, moy.néerl. *stoffe*, qui sont tous, d'ailleurs, relativement récents. Les plus anciennes mentions, à part le français, sont celles de l'angl. *stuff* (depuis le XIV^e^ siècle), qui est à la fois substantif et verbe et a emprunté tous ses sens au français. Tout nous indique que ce mot a son point de départ dans le nord de la France. Kluge et Gamillscheg estiment que le verbe vient du grec *stýphein* 'rétrécir, contracter' et aurait passé de Byzance dans le nord de la France en tant qu'expression propre au tissage de la soie. Cependant, cette hypothèse n'est étayée par aucune preuve matérielle. Il ne faut pas, non plus, partir du substantif, comme le pense Meyer-Lübke 8332, mais du verbe. Celui-ci possède exactement le sens de l'ancien haut allemand *stopfon* 'introduire de force' auquel correspond le germ. occ. **stoppôn* (anc. haut allemand *stopfo* 'choc', *stupf* encore usité dans la Suisse alémanique). Pour les autres mots germaniques apparentés, voir Franck W. s.v. *stop*. Comme Bruckner (Fests Binz) l'a montré, le francique, dans le nord de la France, avait phonétiquement beaucoup d'affinité avec l'anc. haut allemand. L'emprunt étant tardif (peut-être du IX^e^ siècle), le verbe a passé dans la langue avec *-pf-* et non avec le *-pp-* de l'ancien francique du nord. La signification primitive est 'introduire de force'. Elle passe ensuite à 'munir abondamment de' (De là: I). Le substantif dérivé *étoffe* a eu longtemps la signification générale de 'matière dont on doit se munir pour accomplir un travail, etc.'. De là sont sorties, au cours des temps, les spécialisations les plus diverses, dont la plus courante a été 'tissu', tirée elle-même de 'ce qui est nécessaire pour confectionner un vêtement'. Voir encore *typhein* I 2. II 1 est un emprunt récent à l'als. *stopfen* 'gaver (des oies)', 2 vient de l'als. *stoppen*, forme qu'on rencontre aussi en suisse além. (Martin-Lienh. 2,608; Schw Id. 11,1147), qui était en moy. haut allemand. *stoppon* et qui implique un radical germanique *stub-*, variante de **stup-*, d'où *stopfon*.

STŬPPA 'étoupe'

I.1.a. Fr. *estoupe* f. 'partie la plus grossière de la filasse de chanvre' (XII[e] s.—Trév 1752), *estope* (XIII[e] s., Gdf; Huon Regres), *estupe* Studer, *atoupe* (orl. 1391), apr. *estopa*, frm. *étoupe* (dp. Mon 1636), en général dans les dialectes français.

b. Afr.mfr. *estoper* v.a. 'boucher avec de l'étoupe ou qch de semblable, obstruer, fermer en introduisant un objet; barrer (un passage, une rivière)' (Chrestien-StAdrien, Gdf; Mir agn; R 9,246), *estuper* Guernes SThomas, *estupper* Pass Sem, *stopeir* (liég. lorr. XII[e] s.–XIV[e] s., Gdf; Haust Rég 1), fr. *estouper* (vers 1200–Trév 1732), frm. *étouper* (dp. Mon 1636), adomb. *estopar* (Châtillon D. 1385), avaud. piém. *estopar* (AGl 11,297; Muss Beitr 112), apr. id., liég. nam. *stoper*.

2. Afr. *estofer* v.a. 'enlever aux plantes l'air nécessaire à leur végétation' (Metz 1285)[17], mfr.frm. *étouffer* (dp. 1564, Ind Marc 4,7), v.n. 'dépérir (de plantes); s'éteindre (lampe)' Montaigne; *étouffer* des boutures 'placer sous une cloche en verre pour les soustraire á l'action de l'air' (1864–Lar 1870); mfr. *estoffer* 'suffoquer le cœur (moralement)' (hap. XIV[e] s.), mfr. *estouffer* 'tuer' Comm, mfr. frm. v.a. 'faire mourir qn. en arrêtant sa respiration' (Oliv 1535—Trév 1732), apr. *estofar* (XV[e] s.), frm. *étouffer* (dp. Mon 1636), 'suffoquer, empêcher de respirer librement' (dp. Miege 1677), v.n. 'avoir la respiration empêchée, mourir faute d'air' (dp. 1559, Amyot).

II.1. Afr. *clou estoupperol* 'esp. de clou à tête ronde qu'on emploie dans le chevillage des fonds'[22] (Rouen 1382, R 36, 268), mfr. *stoperol* (vers 1520, Jal), *stouperol* (vers 1530, Jal).

2. Apr. *stopin* 'mèche' (1388, Pans 5).

3. Ala *štupuŋ* 'bouchon', Bruzolo, Ostana *stupuŋ*.

Le latin *stŭppa* 'étoupe' a subsisté dans toute la Romania: roum. *stupă*, it. *stoppa*, logud. *istuppa*, engad. *štǫpa*, frioul. *stope*, cat. esp. port. *estopa*. Pour le galloroman voir I 1 a. Moy. néerl. et moy. bas-allem. *stoppe*, flam. *stoppe* (Schuermans; DeBo; Frings GR 102), bret. *stouf* 'tampon' Ped 238, basque *estupa* 'étoupe' sont empruntés par l'intermédiaire de la Gaule. Breton *stoup* 'étoupe' est un emprunt plus récent au français. Comme, de tout temps, l'étoupe a été employée pour boucher des trous, on a dû créer déjà en latin un verbe **stuppare* 'boucher avec de l'étoupe', puis 'boucher (avec une matière quelconque)'. De là roum. *astupa*, it. *stoppare*, anc. lomb. *stopar* AGl 12,434, piém. *stupè*, anc. vén. *stropar* Muss Beitr. Pour le galloroman voir I 1 b. Franck W a montré que la famille germanique de angl. *to stop*, néerl. *stoppen* et allem. *stopfen* n'est pas en relation avec ce mot.

ML 8333 et Gamillscheg ont fait remonter le franç. *étouffer* à **stuppare*, qui

[17] Le texte est: *les espines qui les estoferent* 'les buissons d'épines qui les étouffèrent (les graines)'. Il est tiré de la parabole des semeurs comme me le communique la bibliothécaire de la Bibliothèque municipale de Metz, Mme Marthe Chotin, qui a bien voulu contrôler le passage.

[22] Autre explication, mais sans tenir compte du dernier texte cité, dans Barbier Proc 5,392.

aurait tiré son *-f-* du type **extufare* (voir *typhein*). Combattant cette hypothèse, Brüch ZFSL 52,404, observe avec raison que cet **extufare* n'est pas attesté en galloroman. Il fait remonter *étouffer*, anc. franç. *estofer* (très rare) à l'anc. haut allem. **stopfôn*, forme première du moy. haut allem. *stopfen*. A cette argumentation, on peut opposer que ce verbe subsiste dans le franç. *étoffer*, dans lequel le *ŏ* francique est représenté comme d'ordinaire par *o* (anc. franç. *ǫ*). Il est sémantiquement très voisin des plus anciens représentants du franç. *étouffer*, mais s'en distingue par la qualité de la voyelle radicale (le *ʊ* d'*étouffer* remonte à un *ǫ*); en moy. franç. les significations d'*étouffer* et d'*étoffer* sont totalement différentes. En revanche, *étouffer* a une signification très voisine du franç. *étouper* (I 1 b). De 'boucher avec de l'étoupe' jusqu'à 'étrangler' on trouve presque toutes les significations concuremment dans les deux verbes. Anc. franç. *estǫfer* (= franç. mod. *étouffer*) est donc issu, selon toute apparence, de l'anc. franç. *estǫper* sous l'influence de l'anc. franç. *estǫfer* (= franç. mod. *étoffer*). Les deux verbes *estǫfer* et *estǫper* se sont croisés et ont produit *estofer* (voir I 2). Les textes montrent clairement que le nord de la France est le lieu d'origine de ce type qui ne s'est étendu au sud que vers la fin de la période de l'anc. prov.

Parmi les emprunts (II), 1 est pris à la terminologie des marins de la Méditerranée. De prime abord, on pourrait le rattacher au génois où cet objet s'appelle encore *stoppajêu*. Cependant les textes ne remontent pour ce mot et pour son correspondant italien *stopparolo* que jusqu'au XVII[e] siècle. En revanche, cat. *stoperol* est déjà attesté en 1406 (Jal) si bien qu'il doit être l'origine des mots italien et français. 2 est emprunté à l'it. *stoppino*. Le dérivé en *-one* existe en galloroman comme dans l'Italie Septentrionale (piém. *stopun* 'bouchon', *tupun*, etc.). Tandis que les formes des vallées alpines galloromanes du Piémont sont en général correctes phonétiquement, bien qu'empruntées, peut-être au piém., et assimilées, les formes indiquées sous 3 sont de purs emprunts. ML 8332, 8333; R 53,202.

TOP (anc. francique) 'pointe'

I.1. Afr. *top* m. 'touffe de cheveux au haut de la tête' Eneas, afr. *tup* Gaimar, mfr. *toup* (vers 1180–XV[e] s., Gdf; Artus; Bill).

Dér.—Afr. *toupe* f. 'touffe de cheveux au haut de la tête' (hap.), mfr. *toppe* 'touffe' (bourg. 1419), frm. *toupe* 'touffe de poil sur le front d'un cheval' Wid 1669, 'paquet très dru de cheveux' (Boiste 1803–Besch 1858), Provins 'touffe d'herbe', *tupe*, liég. nam. *toupe* 'touffe (cheveux, herbes, etc.)', Gondc. *tüp* 'huppe de poule', berr. *toupe* 'touffe d'herbes' RD, Gray 'id.; trochet de fruits; houppe de bonnet' Ac Bes 1850, 211, Gr Combe 'houppe'.

Fr. *toupet* m. 'touffe de cheveux au haut de la tête' (vers 1145; dep. env. 1330), 'petite touffe de crin, de laine, etc.' (dp. 1446, Gay), 'réunion d'arbres' (Ac 1718–1798), Esneux *topèt* 'touffe d'herbe' Gesch 274, Roubaix *tüpé* 'touffe de cheveux sur la tête' V 67, Gondc. *tüpéi*, *tʊpẹ́i*, St-Pol *tʊpẹ̆* 'id.; huppe (d'oiseau)', bmanc. *tʊpẹ́* 'chignon', can. *toupet*, bourg. *topô* 'tas, amas' Dur 8,70, Ste-Sabine *topot* 'tas, poignée', HMarne *toupot* 'touffe (d'herbe)' Blaise 305, Gaye 'endroit d'un champ où la récolte pousse plus forte et plus touffue', Brillon 'touffe, petit

tas', Moselle *tǫpę* 'toupet', Vouth. Broye P. Autet *toupot* 'touffe d'herbe', Gray 'id.; trochet de fruits' AcBes 1850,211, Blon *tʊpę́* 'toupet', pr. *toupet*, mars.id. A, Alais id., Mauriac *toupeit*, Chav. *toupé*, lim. *tupet* SPr, bearn. *toupet*.

2. Agn. *topet* m. 'jouet en forme de poire, avec une pointe, qu'on lance à l'aide d'une ficelle enroulée' (vers 1145; hap. XIII[e] s.), *tupet* (1265, R 32,148), *tupeth* Baust Muss 537.

Afr. *topoie* f. (1205, Bodel, R 9,247; flandr. XIII[e] s.), *tourpoie* (lorr. 1312). Mfr. *tourpie* (Froiss; Est 1549–Th 1564), mfr. frm. *toupie* (dp. 1530, Palsgr 282), *turpie* (Dup 1573–Stoer 1625), Malm.nam.Din. Bastogne *tʊpi*, ard. *tʊpy*, Mons *tourpie* (déjà Dl), Maub.id., Valenciennes *torpie*, Nord *tʊrpiy*, Mouscron *toupire*, pic. Seine I. *tʊpiy*, yèr. *toupie*, bnorm. hbret. *tʊpi*, Allier id., *tǭpī*, Nièvre *tʊpi*, *tʊpiy*, bourg.champ. Meuse, frcomt. Mesnay, frb. Vaud *tʊpi*, Meyr. Prades L. Pont M. *tʊpiyo*.

II. Mfr. *rattopper ses debtes* 'couvrir une dette en en faisant une autre, et à des conditions pires' (1567–Cresp 1637, Jun 348).

Cette famille de mots est, à l'origine, uniquement française. Esp. et port. *tope* 'sommet' sont empruntés au franç. En revanche, l'it. *toppo* 'bûche' est emprunté directement à une langue germanique. Ce qui le montre surtout, c'est l'apparition ancienne du mot, et, de plus, dans des significations que le français ne connaît pas et qui, de ce fait, doivent provenir du germanique sans intermédiaire. La plus ancienne attestation, *toppu* 'cocuzzolo di monte' se trouve dans le Codex Cavensis (1064, Agl 15,360). On a ensuite it. *toppo* (attesté en Toscane au XIV[e] s.), apul. du nord *tǫpp* 'colline' AIS 422 (p 717), *tʊppə* (p 708), cal. *toppu* 'mucchio, quantità di roba', piém. *tope* 'ceppo'; puis tosc.sic. *toppa* 'serrure' AIS 885; en outre cal. sic. *tuppu* 'ciuffo di capelli', napol. tarent. abruzz. *tʊppə* 'nodo (di capelli sulla nuca)', abruzz. *toppa* 'touffe de cheveux' (de même nord de la Campanie). La langue d'emprunt ne saurait être le langobard, car, dans cet idiome, si *-pp-* a été conservé, *t-* s'est, en revanche, transformé en *z-*. On doit, en conséquence, supposer que les formes italiennes ayant, en particulier, la signification de 'sommet' sont un emprunt à un goth. **tupp*. V. aussi *TUPP- 'sombre'. L'absence de **tupp* dans les textes gothiques est très probablement une affaire de hasard, puisque le mot est attesté dans toutes les langues germaniques, y compris l'anc.norois.

Cf. moy.néerl. et moy.bas allem. *top* 'sommet, pointe', haut allem. *zopf* 'cheveux tressés' (primitivement aussi 'fin, bout'), anc. frison *top* 'toupet de cheveux', anglosaxon *topp* 'sommet', anc.norois *toppr* 'touffe de cheveux'. En franç., le mot apparaît tout d'abord principalement au sens de 'toupet de cheveux' (1). Sous 2, se trouvent les désignations de la toupie qui ont leur place tout indiquée ici et qui proviennent de ce que ce jouet tourne autour de sa pointe servant d'axe. Cependant il est difficile de décider dans quel pays le mot a été créé. Kluge suppose que cela aurait eu lieu en France où *top* aurait pris la signification de 'pointe'. Ce qui s'y oppose, c'est que le simple *top* n'est jamais attesté en franç. dans cette signification, que l'on trouve, en revanche, dans angl. *top*, flam. *top* (De Bo, Teirlink), moy.allem. de l'ouest *dop*, moy.haut allem. *topf*. D'autre part, sa première mention dans le domaine galloroman est celle d'un texte anglonormand.

Ainsi, il semble que ce soit plutôt l'angl. *top* qui ait passé du sens de 'pointe' à celui de 'toupie' (attesté depuis env. 1060). De l'anglais, la signification a passé, probablement avec l'objet lui-même, en Hollande et en Allemagne. D'autre part, l'anglonormand a repris le mot en le transformant en *topet*. En s'installant sur le continent, ce mot a été modifié par le moyen de suffixes (> *-oie*, puis *-ie*); mais il est difficile de dire ce qui a servi de modèle pour cette opération. *Toupie* a emprunté très vite, et sur un vaste territoire, le *-r-* de *tourbillon* avec lequel il a une ressemblance sémantique. ML 8787, 8788. Il est emprunté à l'ital. *rattoppare* 'raccommoder', dérivé de *toppa* 'morceau d'étoffe, etc., pour raccommoder.'

TOPPIN (anc. francique) 'pot, marmite'

1. Apr. *topi* m. 'pot' (rouerg. vers 1220, Lv; Millau 1422, Doc 281), *topin* (Provence, gasc. XIV^e s.–XVI^e s.), adauph. *tupin*, alyon id. (XIV^e s., R 13,590), afor. id. (1350–1565, Gdf), abress. id. (XIV^e s.–XV^e s., Gdf), asav. id. (1542), mfr. id. (XIV^e s.–Cotgr 1611; Gdf; Rab; BPeriers), *tepin* (Pass Semur; 1571, région de la Loire, Gdf), apr. *toupin* 'grand vase en étain, cuivre ou terre, pour conserver le lait' (HAlpes 1452, Ann Alpes 5,281), St-Hubert *tupin* 'vaisselle', Bouillon id. (1789), *topin* (1789), PCal. *tupin* 'vase quelconque' (1790, RLR 15, 66), Fougères *toupéin* 'pot de terre à 4 ou 5 l', hmanc. *toupin* 'demi-quart de busse, équivalent à env. 30 l', Mâcon *tupin* 'pot en terre', *tepein* 'petit vase de terre pour faire chauffer les liquides' Ragut, bressch. *teupin* 'pot', mouz. *topin* 'vase à boire', *tupin* 'tirelire', Peuv. 'vase quelconque', gaum. 'vaisseau de cuivre', Moselle *tœpĩ* 'pot; marmite à couvercle', *tpĩ*, Fraize *dpi* 'pot en grès', bress. *tepi*, Manocourt *tépin* 'pot' Adam 362, frcomt. *tepin* 'pot dans lequel les cultivateurs mangent leur soupe', *tupin*, Schweiz *toupin*, Blon. *təpẽ*, Ollon *təpãẽ*, sav. *tepin* 'pot en terre ou en faience', *topin*, Ain *tœpẽ*, Ruff. *təpẽi*, Pois. *təpẽ*, Juj. *tupin*, *tepin*, Lyon, Mornant *tupin*, Riveronne, Rive-de-Gier *tïpin*, stéph. *tupin* V, bdauph. HAlpes *toupin* 'pot de terre', Queyr. *tupin*, vaud. piém. *tüpĩn*, NHengst. Serres *tüpiŋ* 'pot, marmite', Pontech. 'vase pour la graisse' AIS 970, pr. *tʊpĩ* 'pot à queue, pour chauffer de l'eau, etc.', Alais *toupi* 'pot à poignée pour potage', Salle SP. 'pot pour faire cuire', Hér. 'pot', Clerm. 'pot à queue', Toulouse 'petit pot de terre' G, Tarn G. Tarn, Lot *tʊpi* 'pot', rouerg. *toupi*, blim. *toupi* 'pot au feu', périg. *toupi* 'marmite', gasc. *toupin* 'petit pot' Pt, Agen *toupi* 'pot de terre; marmite dans laquelle on fait la soupe'. ALF 1065; 715 jatte (sporadiquement); 818 marmite; AIS 955; Fahrh 44; ALLo 1885, 2320; Volkst 6, 27–39; 12,231.

Les mots réunis sous 1 ont leur correspondant exact dans anc.ast. *toppin* 'pignatta' AGl 15,417, piém. *tüpiŋ* 'pentolino' RLomb 49, 1029, monferr. *tupin* 'orinale', lat.méd. *tupinus* 'pignatta' (Piémont, Nigra Cur), anc.ast. *toppina* 'vaso' AGl 15,409, cat. *tupí* 'pot de terre', *tupina*, arag. *tupí*, Alava *tupín* 'marmite de fer à trois pieds', basque *tupín* Z. 17,142, *tupi*, *dupin* 'marmite', *topina;* Krüger HPyr A2,175. Lorsqu'on considère cette expansion du mot et sa présence dans l'ALF 1065, on pourrait être tenté, pour des raisons géographiques, de le tenir pour un emprunt au gothique et au burgonde. Mais il ne peut pas en être question, et cela, surtout, pour des raisons phonétiques. Le mot n'est pas attesté dans ces dialectes et, si on voulait le reconstituer, il devrait se présenter sous la

forme **dupp-* correspondant au néerl. *dop* 'cosse (de pois), gobelet de fer blanc', et au haut allem. mod. *topf*. Cette forme **dupp-* ne peut servir de base pour des formes romanes. Seul entre en ligne de compte un haut allem. **top* qui a été emprunté à une époque où *-d-* avait subi sa mutation, et où *-p-* était resté encore intact. Cette forme subsiste dans le bavarois *der tuppen*. Morphologiquement, les formes romanes correspondent remarquablement au neutre **tupfin* 'récipient' attesté dans les glossaires sous la forme anc. haut allem. *dupfen*, moy. haut allem. *tupfen, duppen*. Une forme correspondante, n'ayant encore subi la mutation qu'en partie, doit avoir pénétré dans le galloroman du VIII[e] siècle environ et s'être rapidement propagée dans le sud de la France et dans les autres territoires voisins de langue romane. Il est permis de supposer qu'une innovation technique faite en Allemagne en aurait été la cause. Dans le galloroman du nord, le mot a d'ailleurs disparu rapidement, si bien qu'il est très rarement attesté et presque uniquement dans la périphérie. Pourtant il s'est maintenu jusqu'aujourd'hui dans presque toute la zone bordière du français: le wallon, le picard, le haut breton, l'angevin, le bourbonnais, le bourguignon, le lorrain, le franc-comtois. La voyelle radicale varie curieusement entre *o, ü* et *e*. Probablement, le *o* protonique de *topin* s'est affaibli en *e*, et celui-ci a passé à *ü* à cause du *p* qui suit.

TUPP- 'sombre'

1. Afr. *estuper* v.a. 'éteindre' Entree, Sampeyre *stüpé*, gasc. *estupá;* Valdieri *štüpiar*, Armagnac *estupí* 'id.; étouffer; atterrer'. Bearn. *estup* m. 'chaleur étouffante' Palay 1,554 b. Périg. *eitufil*[2] m. 'remugle'.—Mdauph. bdauph. *atüpí* 'v.a. éteindre; étouffer; v.n. mourir', Queyr. *atupir* 'éteindre', Puiss. *atupi* 'calmer', bearn. 'étouffer, éteindre; accabler'.—Couzon *entepenó* adj. 'enrhumé'. Stéph. *ontchupinâ* 'prendre un rhume de cerveau'; *ontchupinâgeou* m. 'embarras dans le nez, causé par un rhume'.—Ronco *tüp* 'obscur'.

2. Suisse *topo,-a* 'sourd; taciturne; sombre et doux (du temps)', *fa topo* 'il fait un temps doux', Blon. *topo* 'assourdi (d'un son); couvert (du temps)', Vionn. *topø* 'nébuleux', Wallis *topa* 'obscure (adj.f.)', bagn. *topo* 'sombre, noir', aost. *tœp, -a* 'obscur', *ten teup* 'temps obscur, nébuleux'.

Morv. *étoupi* v.a. 'étouffer en couvrant (le feu)', verdch. 'éteint, bouché (du feu)', Minot v.a. 'étouffer (le feu)'. Mesnay *ètoupi* 'accroupi', Valais *s'atopi* v.impers. 's'obscurcir (du temps)'.—Perreuse *atoupir* 'éteindre (le feu)', centr. 'couvrir pour éteindre; tamponner, étouffer'; Pd'Enh. *s'atoupi* 's'assoupir' Gl 2,81, Aigle *s'atəpi;* Monthey *atəpwei* v.a. 'étourdir', centr. *atœuper*, berr. 'tamponner' RD, bourbonn. 'couvrir, étouffer le feu', Meuse 'couvrir pour cacher ou pour préserver du froid', neuch. *atopa* 'engourdir, rendre somnolent'.—Mfr. *entoupi* 'engourdi, endormi (d'un loir)' (Lyon 1545, R33,348,557; Nph M 41,104); Albertv. *eintopâ* 'tête enveloppée par suite de douleurs névralgiques ou autres', *êtopa*.

Cette famille de mots a été reconstituée pour la première fois par Jaberg, Arch.

[2] La terminaison indique nettement un ancien **estupit* quoique le mot ne soit plus attesté, par ailleurs, en périg. De même, le *-f-* provient des représentants occitans du franç. *étouffer*.

132,224, qui ne l'a, au reste, discernée qu'en piém. et en franco-prov. Jud, RLiR 1,194, a attiré l'attention sur l'extension ultérieure et sur l'inégalité de la voyelle tonique. 1 représente un radical **tŭpp-;* 2, un **tŭpp-*, soit **tǫpp-*. Le fait que 1 est largement répandu, en partie sporadiquement, montre bien qu'il est le type primitif. Le texte de l'Entree, ouvrage qui contient tant d'italianismes est un témoignage en faveur de la présence ancienne de ce type dans l'Italie Septentrionale.[4] Pour le franco-prov., 1 est attesté par le stéphanois *ontchupinâ;* pour le piém. limitrophe par *stupilè* 'chiudere ermeticamente' et par Limone *tŭpir* 'éteindre' AIS 921 p. 182. Au-delà de l'occitan, où il est très répandu, nous le retrouvons dans esp. et dans port. *tupir* 'comprimer, bourrer', *entupir*, et port. *atupir*. 2 se trouve dans piém. *tup* 'sombre', Val Sésia *tupp*, Val Maggia *top*, en outre (v. plus haut), en franco-prov., en bourg., en bourbon., en berr., et, isolément, en lorrain. C'est manifestement un domaine plus étroit dans lequel le type primitif a modifié sa voyelle par des influences secondaires. Jud rapporte avec raison cette influence au verbe **stuppare* 'boucher avec de l'étoupe' (voir *stuppa* I 1 b). Ce verbe a une grande vitalité dans toutes les régions où 2 apparaît. Leurs significations sont très voisines et ils sont phonétiquement identiques à part la légère différence de la voyelle et le *es-*, dont les sujets parlants ont le plus souvent le sentiment qu'il est un préfixe. Il n'est pas toujours facile de dire où l'on a un pur représentant de **stuppare* ou un dérivé de **tŭpp-* ayant seulement subi l'influence de **stuppare*. En premier lieu, les verbes en *-ir* (*étoupir*, *atoupir*), comme dérivés d'un adj. remontent à **tupp*. Il en est de même d'*atouper* qui est géographiquement voisin d'*atoupir* et lui correspond sémantiquement. Au reste les deux groupes 1 et 2 se correspondent souvent dans les dérivés avec exactitude. Cf. par ex. le stéphanois *ontchupinâ* et le vaudois *eintoupenâ*. **tupp* est probablement prélatin;[6] d'ailleurs, on ne peut actuellement le rattacher à rien. Le *toupek* 'sombre' du patois de Vannes (avec *ou*) lui est fort voisin. Peut-on conclure de ce fait à une racine celtique?

TOPP- (onomatopée)

I.1. Afr. *toper* v.a. 'appliquer à, placer en jetant' Thebes, mfr. 'appliquer, placer' (champ. 1382), Roanne 'cogner, battre', ang. *touper* v.n. 'faire ressentir

[4] Littré cite encore un passage de Froissart où se trouve un *estuper* 'éteindre'. Cependant l'édition de Luce 10,231 ne confirme pas cette leçon. Selon cette édition, les manuscrits ont *estoupper* ou *estouffer* (et encore *esconser*, qui donnerait un sens un peu différent, et *eschaufer*, qui est dû à l'étourderie d'un copiste).

[6] Une question reste encore ouverte, celle de savoir si cette famille de mots est en relation avec une autre famille certainement préromane elle-même, avec les voyelles *u* et *i*, qui existe en Sardaigne et dans l'Italie du sud: logoud. campid. *tuppa* 'fourré', logoud. *istuppu*, Nuoro *attuppare* 'pousser dru (des céréales)', logoud. 'se cacher dans la forêt'; campid. *tippiri* 'pousser dru (des céréales)', *tippiu* 'épais', sic. *tipu* 'bourré, rempli', *attippari* 'bourrer'. Esp. *tupir* 'comprimer' doit être placé aussi dans ce groupe. Voir Wagner Leben 8. Il est certain que ce groupe de mots a rencontré dans la péninsule italique celui de l'ital. *toppo* 'bûche', d'origine probablement gothique, avec lequel il s'est, en partie, tellement mêlé qu'une séparation nette serait difficile à obtenir. V. en outre anc. basfrancique TOP.

des élancements douloureux; gronder avec des paroles véhémentes', Mâcon 'causer des élancements douloureux', Igé v.n. 'heurter la tête contre qch.'.

Comp.—Gondc. *žü d'top* 'jeu de croquet sur table avec des arceaux, autrefois, dans un cabaret', être *top a top* 'à égalité de fortune (de deux fiancés)'.

2. Frm. *top* m. 'cri pour prévenir les aides de noter l'indication du cadran, à ce moment précis, chaque fois qu'il s'agit de faire des observations simultanées' (dp. 1877).

II. Frm. *top et tenc* 'j'accepte (le défi)' (t. de jeu, Mén 1650), *tope et tingue* (Oud 1660—Ac 1878), m. 'sorte de jeu de dés' (dp. Ac 1762; «vieilli» DG), 'interjection employée entre buveurs, lorsqu'on accepte un défi de boire ou une santé qu'on porte' (Wid 1669–1675), *tope* (OudC 1640—Trév 1771), 'interjection qui exprime l'acceptation d'une offre (jeu de dés)' (Wid 1669—Ac 1878); 'volontiers (interj.)' (dp. 1674, La Font), *tope là* (dp. Lar 1876); *toper* v.n. 'accepter l'enjeu de l'adversaire (au jeu); v.n. adhérer à une proposition' (dp. Dorimon 1659), 'frapper dans la main en signe d'accord' (dp. BL 1808), pic. 'frapper de la main pour conclure un marché', boul. 'toucher' Ds, St-Pol, Bellême 'consentir à une proposition', Suisse 'conclure un marché', mars. *topo!* 'soit!; mot de reconnaissance des garçons du devoir' A, frm. *toper* 'questionner un compagnon qu'on rencontre' Delv 1867, Paris 's'aboucher avec qn.' B, mars. *toupar* 'consentir à une proposition' A; Jers. *toper* 'trop boire'.—Moyen franç. *touppebras* m. 'balancine' (Rouen 1382)[1].—Anc. franç. *soi atouper a* 's'attaquer (à qn)' (Blois vers 1250); moyen franç. *attoupper* v.a. 'toucher (qn) indiscrètement, frapper' (1537).

Cette famille de mots a des correspondants dans plusieurs langues.[3] Ainsi bas allem. *topp machen* 'conclure un accord', *topp halten* 'respecter un contrat', moy. bas néerl. *toppschilling* 'arrhes payés à la conclusion d'un contrat' (1351), esp. *topar* 'rencontrer qq., heurter' (déjà Juan Ruiz), *tope* 'choc, rencontre', *topetar* 'heurter', cat. *topar*, *retopar* 'repousser', galic. *atopar* 'rencontrer', basque *topa*. Le haut allem. mod. *topp* (interjection, depuis 1696) est emprunté au bas allem.

L'origine de ce mot est probablement une formation onomatopéique qui sert à rendre le bruit court et sourd de deux objets qui se heurtent ou de la poignée de main de deux personnes qui concluent une affaire. Les formes françaises (sous I 1) pourraient être empruntées au néerl. Mais le verbe est attesté trop anciennement, si bien que, là aussi, il est probable que le mot a été formé par imitation du bruit. 2 est vraisemblablement indépendant de 1 et a été créé onomatopéiquement, peut-être suggéré par l'interjection *stop*.

Les mots réunis sous II sont issus du terme de jeu donné au début et qui est

[1] Barbier Proc 5,330 voit dans cette forme une modification d'un moyen franç. **touperas*, qui serait un composé de deux termes empruntés au néerl.: *toppen* 'donner aux vergues une position verticale' et *ra* 'vergue'. Mais le fait que le néerl. *toppen* n'est attesté que depuis le XVII[e] siècle (l'angl. *to top*, qui lui correspond, est attesté avec certitude depuis 1549) s'oppose à cette hypothèse. Ce mot doit être un composé du verbe anc. franç. désigné ci-dessus et de *bras* 'espèce de corde'.

[3] Un anc. franç. *toupe* est traduit par 'coup' dans Til Glan. Cependant, la signification de ce mot, qui n'est attesté que dans Moamin, est incertaine. Ce texte est, en effet, parsemé d'éléments étrangers. La forme, d'autre part, est écrite tantôt *toupe*, tantôt *tupe*. On ne peut donc tenir compte de ce mot ici.

lui-même emprunté à l'expression espagnole *topa y tengo*. II a absorbé **talpa* I 2 a qui lui ressemble phonétiquement et lui est presque identique sémantiquement.

TOPPEN (néerl.) 'apiquer'

Composé fr. mod. *toupras* m. 'amarre de terre d'un vaisseau' (1848, Terre-Neuve).

Les deux parties du composé, *toppen* et *ra* 'vergue' ont certainement passé du néerlandais en français grâce à la rencontre des pêcheurs des deux nations dans les eaux américaines. Un emprunt antérieur n'est pas vraisemblable. Voir *topp-*. Barbier Proc 5,330.

**TALPA 'patte'

I.1. Abern. *tarpe* f. 'grosse patte d'animal (ours, etc.)' (1360), Châten. *târpe* 'id.; grosse main d'homme', Montbél. *tarpə* 'large patte', Bourn. *tārp* «grosse main», Aj. *târpe*, neuch. *taupe* 'patte d'ours' (dp. XVII^e^s.), 'grosse main; mitaine', Vaud *tarpa* 'patte d'animal à griffes'; sav. Alex 'tache; macule'.

2.a. Argot *tauper* v.a.n. 'battre, accabler de coups' (Delv 1867—Del 1896), Plech. *tawpə* 'frapper', ang. *tôper* v.a. 'attaquer, interpeller; pincer, prendre en défaut', louh. 'frapper qn.', Dijon, frcomt. *tauper*, PtNoir *tôpè*, GrCombe *s tǫ̃pẽ* 'se battre', Suisse *taupá* 'battre, rosser', *tauper*, Blom. *tǫpá*, Thônes, BalmeS. *tôpâ*, béarn. *taupa* 'toucher, palper; toper'.

b.Frm. *tauper à qch.* 'consentir à qch., approuver qch.' (1616—Trév 1771, Anc Théât; «surtout t. de débauche et de jeu» Wid 1669; Brunot 3), *tôper* (Retz—Ac 1835), *dire tôpe à* Sév, *tauper dans* (1792, Brunot 10), *taupe et tinc* 'de tout mon coeur, volontiers' (Trév 1704—1752), *taupe et tinque* Trév 1771, *taupe et tinc et grand merci* 'je le veux et je vous en suis obligé' (Trév 1704–1752), *taupe et tinque et grand merci* Trév 1771, Ancenis *taupe! . . . botte!* 'à votre santé . . . à la vôtre' Gaumer, ang. *tôper* 'v.n. se frapper réciproquement dans la main pour conclure un marché; v.a. approuver, consentir, soit en frappant dans la main de son interlocuteur, soit en trinquant', *tôpe* interj. 'à votre santé'.

II. Metz *talpé* m. 'servante malpropre'. Courtelary *talpè* 'individu maladroit'.

Les formes frcomt. et francoprov. données sous I 1 correspondent au suisse além. voisin *talpe* 'patte, griffe'. Mais elles ne peuvent pas avoir été empruntées tardivement à cause de l'évolution de *-lp-* en *-rp-*. *-rp-* indique un ancien *-lp-*. Ce même type **talpa* 'patte' doit être aussi à l'origine du verbe *tauper* qui est très répandu en français et dont le domaine s'étend jusque dans la partie occidentale de l'occitan (2a). Dans la signification de 'accepter ou confirmer qch. en frappant dans la main' (b) le verbe a été d'ailleurs rapidement remplacé par *tope*, *toper* (voir *topp-*) empruntés à l'espagnol ou, pour mieux dire, il a été absorbé par ces mots introduits nouvellement à cause de la ressemblance phonétique et l'identité sémantique qui existe entre les deux familles de mots.[2] En somme,

[2] Il ne serait pas exclu que l'influence du terme de jeu espagnol *topa* fût le point de départ de tout ce complexe (voir I 2 b et *topp* II). Dans ce cas, l'évolution aurait été la suivante: esp. *topa*, *topar* auraient été introduits en France avec le jeu et, là, ils auraient été

dans la plus grande partie du domaine galloroman (excepté le picard, le normand, l'Ile de France, le champenois, la partie orientale de l'occitan), existe un mot **talpa* 'patte', qui est resté comme substantif dans I 1 et est attesté, dans I 2, par le verbe dérivé tandis que le substantif dans ce sens a disparu ici. D'autre part, on a encore à Comelico et à Portogruaro *talpa* 'patte', et à Cormons, donc dans le Frioul *talpe*. Ici le développement sémantique est analogue à ce qui a eu lieu dans le galloroman: frioul. *talpe di ors* 'heracleum sphondylium', etc., *talpade* 'impronta d'una zampa', (comme I, 1), *talpâ* 'zampare, calpestare', *talpinâ* 'calcare coi piedi camminando', *talpetâ* (correspondant morphologiquement à I 2), *talpòn* 'ignorante, grossolano', etc. De plus, le mot est très répandu dans l'ouest et le sud-ouest des pays de langue allemande: suisse além. *talpe* 'patte' (depuis le XV[e] siècle), souabe, bad. als. *talpe*, Haut-Palatinat *talpen*, thüring. *talpen* 'marcher lourdement'; peut-être Westphalie *talps* 'lourdaud', Altmark *talpsen* 'frapper fort avec le talon'. En outre, en terre de colonisation allemande, Saxe *talpe* 'grosse main' (Müller-Frauenreuter 1,189), *talpen* 'manier maladroitement', Silésie *talpe* 'main' Weinhold. Ce mot manque complètement en Bavière et en Autriche et n'a pas de correspondants dans les autres dialectes germaniques (anglosaxon, germ. du nord, goth.). Cette revue géographique montre que les Germains ont très probablement pris ce mot à la langue des anciens habitants et qu'en galloroman aussi il provient d'une langue prélatine. Tagliavini Z.46,51 le décompose de la même manière que *talpa* 'taupe' en un élément *tal-* 'surface, terre' et un suffixe *-pa*, mais sans tenir compte des formes allemandes et galloromanes[3]. Le domaine géographique de ce mot permettrait de croire à une origine illyrienne. Mais la question de savoir pourquoi les consonnes de **talpa* n'ont pas subi de mutation en allemand reste.[4]

*TOPF (anc. além.) 'touffe de cheveux'

Afr. *tofe* f. 'gros flocon de neige' (pic. vers 1180), fr. *touffe* 'petit bosquet d'arbres serrés' (hap. XIII[e] s.; Th 1564—Lar 1876), 'ensemble d'herbes sur pied, de fleurs naturellement rapprochées' (1393; dp. Est 1538), *toffe* 'éponge de rivière' (Th 1564—Cresp 1637), *touffe* 'ensemble de cheveux' (dp. Est 1539), *tuffe* Desgr 1821, *touffe* 'partie d'un arbre qui donne de l'ombre' (Enc 1765—Lar 1876),

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tout d'abord transformés sous l'influence de *TALPA I 2 a en *taupe, -er*. Secondairement, grâce au contact permanent des joueurs français avec les Espagnols, la voyelle de l'esp. *topa, -ar* se serait aussi imposée en franç. et aurait supplanté le *-au-* de *taupe, -er*. Cette explication est rendue assez vraisemblable par le fait que la forme avec *-ō-* s'est glissée même dans la locution *tope et tingue*. Les dictionnaires permettent de reconnaître nettement les phases de ce fusionnement. Voir Rich 1680: on prononce *tôper*; Trév 1704: la prononciation ordinaire de ce mot approche plus de *tauppe* que de *tope*; Littré: plusieurs prononcent *tôper*; Besch: tout le monde prononce *toper*.

[3] On doit écarter le magyar *talp* 'plante du pied', dont le dérivé *talpas* 'au large pied' est devenu le sobriquet des fantassins hongrois, d'où est emprunté l'allem. *tolpatsch*. Le mot magyar est en rapport avec le finn. *talla* 'lamelle', *tallea* 'fouler aux pieds'. Le roum. *talpă* 'semelle' est emprunté au maggyar.

[4] Un prototype **dalba*, qui expliquerait les formes allem., ne peut pas, d'autre part, s'accorder avec les formes romanes.

'genre d'agaric' (Boiste 1823–1829), Provins *tuffe* 'touffe d'herbe', PCal. *tǫ̃f* 'mèche de cheveux', *tvf* (p 274), St-Pol *tǫ̃f* 'trochet (de certains fruits)', yèr. havr. *touffle* 'touffe', vosg.mér. *tŏf* 'mèche de cheveux', Ste-En. *tvfo* ALF 829; Bloch Atl 483; ALLo 529. Castr. *touflo* 'cépée'.

Dér.—Mfr. *toffel* m. 'touffe (d'herbe)' (1420), *touffeau* (1594; Hrd Ta 251), 'petite touffe (de cheveux)' (Est 1538—Cotgr 1611; Oud 1660), norm. *toufel* 'bouquet' MN.—Mfr. *tuffu* adj. 'qui est en touffe épaisse' (1438), mfr. frm. *touffu* (dp. Est 1549), *touffu* (de) 'garni (de poils, de feuilles)' (Paré; Pin 1562).

ML 8973 rattache au latin *tūfa* ce groupe de mots limité à la partie septentrionale du domaine galloroman et voit dans la voyelle, et même dans le *-f-*, l'influence de la famille de *buff-*. Cependant, ce groupe est phonétiquement si éloigné de ce que *tūfa* aurait donné régulièrement au nord de la Gaule qu'un croisement n'aurait guère eu de chance de se produire. Gamillscheg veut ramener *touffe* au néerlandais *tuife* 'toupet' que je ne trouve attesté nulle part et que Valkh ne connaît pas non plus. De même, un anc. bas-francique **tupfa* 'tresse', que Diez 689 avait proposé, n'a, vraisemblablement, pas pu exister parce que, dans ce dialecte, *-pp-* n'a pas subi la mutation. D'autre part, un **topfa* est à peine imaginable dans les dialectes franciques du sud parce que *t* y a été modifié aussi bien que *p*. En revanche, Brüch RLiR 2,81 estime avec quelque vraisemblance que la modification de *p* a eu lieu dans l'anc. alémanique avant celle de *t*. Donc, pendant un certain temps, entre l'ancien état **topp-* et l'état ultérieur *zopf*, on aurait employé une forme **topf*. Ce serait de cette forme de l'anc. alémanique et à l'époque où elle existait, soit au VIII[e] siècle, que **topf* aurait été emprunté par le galloroman du nord. Ce mot serait, avec *raus* (> *roseau*) un témoin d'une certaine infiltration alémanique.

TŪFA (lat.) 'bouquet de plumes servant de signe de reconnaissance à la guerre'

Lang. *tuffe* f. 'huppe de l'oiseau' Borel 1655, Péz. Puiss. *tufo* 'id.; toupet de cheveux au milieu de la tête', Toulouse 'touffe de cheveux; ancienne coiffure de femme' G, Tarn 'huppe', castr. 'touffe de plumes de certains oiseaux; tête de cochon, hure de sanglier', Cahors 'houppe de plumes; tignasse', lim. 'huppe d'oiseau' DD, périg. 'chignon relevé', béarn. *tufe* 'touffe de cheveux des femmes; touffe', *touhe;* mdauph. *tūfo* 'mine renfrognée, visage sombre; dépit, frayeur'.—Dér. Tarn, castr. St-Sernin *tufat* adj. 'huppé'.

Le latin *tufa* n'est attesté qu'à la fin du IV[e] siècle chez Ammien Marcellin et chez Végèce. Il a, chez le premier, cette définition: «genus vexilli ex confertis plumarum globis». Il est apparenté à l'anglosaxon *þūf* 'insigne militaire' (*þuuf* chez Bède). C'est, sans aucun doute, un mot germanique introduit par les mercenaires germaniques. Il subsiste encore dans roum. *tufă* 'buisson, bosquet', albanais *tufə* "rameau feuillu", Pistoia *tuffazoli* 'bigoudi, papillotte', Lucques *stufaccioli*, Versilia *stufazzo* 'poignée d'herbe', peut-être aussi dans galic. *tufa* 'jonc', de même dans anc. esp. et portug. *trunfa* 'toupet' (Steiger 47) ainsi qu'en occitan (voir plus haut). Voir encore **topf*. Diez 338; Puşcariu; ML 8973; Brüch.

Université de Bâle

PERSPECTIVES AND PROBLEMS OF AMERINDIAN COMPARATIVE LINGUISTICS

MORRIS SWADESH

> Were vocabularies formed of all the languages spoken in North and South America, preserving their appelation of the most common objects in nature, of those which must be present to every nation, barbarians or civilized, with the inflections of their names and verbs, their principles of regimen and concord, and these deposited in all the public libraries, it would furnish opportunities to those skilled in the languages of the old world to compare them with the new, now or at any future time, and hence to construct the best evidence of the derivation of this part of the human race.—Thomas Jefferson (in Notes on the State of Virginia)

0. PURPOSE. The earliest pioneers of American Indian comparative linguistics understood the value of this study in clarifying the prehistory, recent and remote, of the New World. This understanding motivated and guided the efforts of Jefferson and Du Ponceau, of Gallatin, of Brinton and Powell, of Uricoechea and Belmar, of Rivet, Dorsey, Gatschet, Boas, Swanton, Dixon, Kroeber, Michelson, Sapir, and of many others in earlier periods and today. At times, some scholars despair of solving the difficult problems of remote prehistory and confine themselves to details of historical phonology or to the compilation of descriptive materials. The continued progress of Amerindian comparative studies owes thanks to all the workers in the field. Little could be accomplished without the painstaking detailed concentration on small component problems, but it is also well from time to time to reexamine the broad questions in the light of accumulated data and understanding, so that we may be better guided in our work. The present paper attempts to do this in connection with a series of comparative problems, of the relatively difficult sort, which have come to the author's attention in the course of both narrow and broad research in the Amerindian field.[1]

The problems of Amerindian comparative linguistics in their general nature are the same as those of other areas, with certain special features. One of these is the almost total lack of ancient historical materials, with Mayan hieroglyphs as the one exception. Another is the great linguistic diversity: there are probably far more than 1000 distinct Amerindian languages, and, if classified into groups comparable to the Germanic or Slavic, there would probably be 200 or more—counting, of course, isolated languages not assignable to such families. Still another feature is the relative emphasis which must be placed on long-range comparison in order to solve problems of prehistory. On the other hand there is a definite limit on the time-depth, resulting from the fact that the continent was first peopled about 15,000 years ago. If comparative linguistic science, either in America or elsewhere, should develop a reliable methodology capable of

[1] This paper was begun in 1952 with the help of a grant from the Social Science Research Council of Columbia University and finally completed as part of a project assisted by the Penrose Fund of the American Philosophical Society.

delving thus many thousands of years into the past, then any further penetration would necessarily relate American languages with Old World tongues, according to the vision of Thomas Jefferson as cited at the head of this article. Some of the languages may, however, be of more recent arrival and therefore capable of being related to the Old World by means of methodology already developed. These probably include Nadene, which is evidently related to Sinotibetan, and Eskaleutian, which may be related to Indoeuropean or Uraltaic or both.[2]

The needs of Amerindian comparative linguistics include first of all data, then theoretical understanding, improved methodology, and the long patient labor of applying the best comparative methods to the data. The collection of material on America proceeds slowly, and the publication of the data lags even far behind the collection. There is hardly a linguistic family or stock in America which a comparative linguist can undertake to study without first solving the problem of obtaining the descriptive raw material. To overcome in some measure this major obstacle, the author recently began the collection and publication in mimeograph form of a series of American vocabularies, covering a list of 200 selected noncultural items, along with data sheets outlining the phonetics and principal structural features of each language.[3] The first hundred of these vocabularies, prepared by many scholars and missionaries, now collected and in process of publication, have suggested a number of problems and given hints for answering some of them. The solutions suggested in this paper are generally speaking far from final. Whatever value they may have is probably more in the way of suggesting possible improvements in methodology than in providing final answers to questions of American prehistory.

1. Attainable Time-Depth. Languages change slowly but surely, so that whenever we have a preserved record of a modern tongue as it was spoken 1000 years ago we find easily perceptible and sometimes very striking differences in its present phonetics, structure and vocabulary. If a language changes somewhat in a thousand years, what will happen in 15,000 years? If the first immigrants into America that far back all spoke a single language and if that language split up as the people themselves spread out and formed separate communities out of contact with their old linguistic kin, the result today would be hundreds of distinct languages, some of them very far removed from each other. What are the chances of rediscovering traces of linguistic kinship in languages which have been diverging for so long a time? If one finds apparent traces, what are the possibilities of scientifically demonstrating the original common origin?

The best developed field of comparative linguistics is probably Indo-European

[2] See Robert Shafer, Athapaskan and Sino-Tibetan, *IJAL* 18.12–19 (1952); discussed by Morris Swadesh, *IJAL* 18.178–181. A. Sauvageot, Eskimo et Ouralien, *Journal de la Société des Américanistes de Paris* 21.296–7 (1924); William Thalbitzer, Possible Early Contacts between Eskimo and Old World Languages, *Selected Papers of the XXIXth International Congress of Americanists* 50–54 (1952); L. L. Hammerich, Can Eskimo be related to Indo-European? *IJAL* 17.217–23 (1951). Also references cited in these works. These theories could be tested by methods discussed in the present paper.

[3] Morris Swadesh, ed., Amerindian Non-Cultural Vocabularies, 1954– (mimeograph publication).

and Indo-Hittite. Here, with the aid of historical records, which in some cases date back over 2000 years, scholars are able to fully establish relationships going back four or five thousand years.[4] In Hamito-Semitic, aided by records of several millenia ago in Babylonian and Egyptian but much more recent or nonhistorical for some Semitic, Berber and Cushitic languages, connections have been established which considerably antedate ancient Egyptian and which may therefore hark back eight or ten thousand years. Probably similar time-depths have been attained without the aid of historical material in some areas, including North America. But 15 millenia is a great deal more than 10.

Paul Radin's effort to show the genetic unity of North American languages[5] suggests a means of testing whether common origin is still discernible after such a long lapse of time. In fact, if Radin had used somewhat stricter standards of phonetic similarity and if he had included Central and South American languages, his paper might well have succeeded in its purpose. The list below attempts to correct the weaknesses of Radin's. It includes only such sets of possible cognates as show more than a single common corresponding sound, and preferably those in which at least a consonant-vowel-consonant (CVC) sequence seems to agree. However, if some of the compared languages show CVC agreements, then other languages showing less substantial forms, which could of course be reduced from a more complex one, are also shown. No set of presumptive cognates is included unless it involves at least five distinct linguistic stocks in both North and South America.

Where possible, reconstructed proto-forms for a family or stock are given, based upon contemporary words in two or more languages of the grouping; such forms are starred. The reconstructions are generally provisional suggestions, rather than the product of a fully developed phonological theory, but they should be sufficiently accurate for the purposes of a preliminary assay.

In judging the significance of the comparisons, their quantity and quality should be considered in the light of the data from which they were derived. The most systematic portion of the material was about 120 standard word lists representing about 40 families. They are mainly of North and Central American languages, with only about nine complete lists from South America. Scattered and often very fragmentary data on various other languages were also used. Main attention was given to 97 non-cultural items of meaning which make up the diagnostic test discussed elsewhere in this paper.

The classifications used in grouping the languages are believed to be fairly accurate for North America, constituting in each case well-defined families or stocks, but the arrangement for much of Central and South America is provisional.

[4] See Marija Gimbutas, On the Origin of North Indo-Europeans, *American Anthropologist* 54.602–611 (1952) and Morris Swadesh, Archeological and Linguistic Chronology of Indo-European, *AA* 55.349–352. Both these articles deal with only part of the stock, showing less than the full time-depth.

[5] Paul Radin, The Genetic Relationship of the North American Indian Languages, *University of California Publications in Am. Arch. and Ethn.* 14.489–552 (1919).

Because of the great value of uniform alphabets in facilitating comparison, the spellings in the various sources have been modified according to a single system. Symbols requiring explanation are: *ɬ* voiceless spirant lateral (approximately English *thl*), *c* shibilant affricate (*ch*), *ȼ* sibilant affricate (*ts*), *ɟ* voiced shibilant affricate, *ʒ* voiced sibilant affricate (*dz*), *ƛ* lateral voiceless affricate (*tl*), *ʎ* palatalized voiced lateral (Spanish *ll*), *x* palatal fricative, *χ* post-palatal fricative, *ph th* etc. aspirates, *ṗ ɬ̇* etc. glottalized; *ɨ* unrounded central or back vowel, *ɸ* rounded front, *aⁿ eⁿ* etc. nasalized. When no contrast exists, *c* may be used in place of *ȼ*, and *s* in place of *ʃ*. Similarly *an* may be used in place of *aⁿ*.

'man or person'

Kwakiutlan **wism* (Kwakiutl, Bella-bella)
Coast Salishan Nooksack *s-wiqa* 'man', *s-waqθ* 'husband'
Sahaptian **winc* (Sahaptin *ʔinʃ*, Molale *wicp*)
Wintunian **win* (Wintu *wintuu*, Nomlaki *wuntuun*, Patwin *pat-win*)
Mayan **winiq̇*
Siouan Dakota *wicha*
Caddoan **wiiȼ* (Wichita *wiiȼ*, Pawnee *piita*)
Tumuqua *wiro*
Arawakan Lokono *uadili*, Dominica *uokuli*
Cariban Galibi *uokuli*
Panoan Caxinaua *ɨna*
Mosetino *uenci* 'husband'
Quechua *runa*
Mapuche *wenʈu*
Yamana *win*

'mouth'

Kwakiutlan **sms*
Sahaptian **sim* (Klamath *sum*, Molale *similk*, Nezperce *him*, Sahaptin *ʔ*m)
Maiduan **sim*
Yokutsan **sama*
Siouan Catawba *sima*
Quechumaran **simi* (Quechua *simi* 'mouth', Aymara *simiya* 'lip')
Tehuelchean Tehuelche *ʃam* 'mouth', Ona-Haus *ʃem* 'lip'

'neck'

Algonkian Shawnee *kweyka*
Chemakuan Quileute *-q̇us*
Kwakiutlan Kwakiutl *q̇uq̇uɬa*
Salishan **kspan*
Takelma *kwen-*
Coosan Hanis *kwinc*
Yokutsan Yauelmani *ʔookun*
Zoquean **kini*
Mayan **qʷil* (Yucatec *kal*, Cakchiquel *qul*)
Zuni *kisa*
Utaztecan **kuhca*
Kiotanoan Tewa *kee*
Chitimacha *ke*
Atakapan **kuy*
Yuchi *kho*
Caddoan Pawnee *kiiȼu*, Wichita *kitiȼa*
Arawakan Dominica *igɨnao*
Chibchan Chibcha *gikihca*
Quechumaran **kunka*
Puelche *akwal*
Pilaga *-kosot*

'give' or 'hand'

Algonkian **miiθ-* 'give'
Totonacan **miki*ʔ 'hand' (Tepehua *miki*ʔ, Totonac *makan*), Totonac *maaʃkii-* 'give'
Zoquean *moʔa* 'give'
Atakapan **mis* 'give' (Calcasieu, Hiyekiti)
Salinan *menen* 'hand', *mak* 'give'
Comecrudo *mapi* 'hand', *ayema* 'give'
Seri *moʃi* 'give'
Chontal *ʔamaane* 'hand'
Muskogean **ima* 'give'
Kietanoan **mɛɛʔ* 'give', **may* 'hand'
Utaztecan *maka* 'give', **mayi* 'hand'
Chibchan Chibcha *mis-* 'give'
Quechua *maki* 'hand'
Mosetino *somei* 'give'
Panoan Caxinaua *mikin*
Tupian Guaraní *meʔe* 'give'
Tehuelchean Ona *mar* 'lower arm', *kis-marei* 'give'
Yahgan *marpo* 'hand'

Also connected may be many words for 'five'; possibly also 'four', since this

is the count of the fingers without the thumb, e.g. Salishan **mus*, Wakashan **muw*, Tunica *man-ku*, Chitimacha *meʃa*, Atakapan Calcasieu *himatol*, Zoquean Zoque *makʃkuˀy* etc.

'hand'

Sahaptian Sahaptin *q̓mkas*
Miwokan Lake *k̓upum* 'finger'
Mayan **q̓ab* or **q̓am-paq* ('hand-bone')
Zoquean **kiˀi*
Totonacan **mikiˀ* (see also preceding set)
Huave *-kwal*
Zapotecan **kaˀyu*
Arawakan Lokono *akabo*, Dominica *ukabu*
Cariban Goajiro *ahappu*
Mataco *kwe*
Mapuche *kwi*

'stone', secondarily 'hard, big', and also 'tooth'

Coast Salishan Chehalis *tawɬ* 'big'
Tillamook *tn* 'big'
Wakashan Kwakiutlan **l̓ism*, Nitinat *l̓idickʷ* 'stone' (regularly *d* from *n*)
Sahaptian **tut* 'tooth' (Klamath *tut*, Nezperce *tit*)
Takelma *tan* 'stone'
Wintunian **sii* 'tooth' (perhaps from **tii*)
Kalapuyan **taa* 'stone'
Totonacan **tiɬi* 'tooth' (Totonac *taɬan*, Tepehua *tiɬilaat*), Totonac *ƛankaˀ* 'big'
Zoquean **tiɬ* 'tooth', Oluta *taˀanik* 'big'
Mayan **ton* 'stone'
Huave *-tang* 'big'
Utaztecan **tema* 'stone,' *tama* 'tooth'
Tunica *tika* 'big'
Muskogean **tali* 'stone'
Siouan **thon* 'big' (Dakota *thanka*, Osage *tonga*, Ofo *iton*), Catawba inti 'stone'
Yuchi *thi* 'stone'
Caddoan Wichita *taɬiˀi* 'big'
Otomian **do* 'stone', Tenango *doho* 'big'
Arawakan Dominica *dobu* 'stone'
Tupian Guaraní *tuvica* 'big', *taiⁿ* 'tooth', *ita* 'stone, Cocama *tua* 'big', *itake* 'stone'
Moseteno *derci* 'big' *dondo* 'stone, chalk'
Quechua *hatun* 'big'
Mataco *tunte* 'stone', *tun* 'hard'
Pilaga *-odaite* 'tooth'
Tehuelchean Tehuelche *den* 'tooth', Ona *ton* 'big'
Yahgan *tun* 'tooth', *atuc* 'bone'

'belly'

Algonkian Shawnee *-pehwata*
Tsimshian *pen*
Sahaptian Molale *puuwi*
Kalapuya Santiam *p̓aw*
Miwokan Lake *puluk*
Yokutsan **palik̓*
Zoquean Oluta *puˀpu*
Totonacan **pihin* (Tepehua *pihin*, Totonac *paan*)
Utaztecan **puʃ*
Kiotanoan Kiowa *buut*
Caddoan Pawnee *pakus*
Otomian Tenango *foho*
Cariban Galibi *potosu*
Panoan Caxinaua *posto*
Betoya *apue*
Mapuche *pwe*

'hair, fur, feathers', secondarily 'wing'

Algonkian Shawnee **holeekwa* 'wing'
Bellacoola *-alaˀk* 'feather'
Sahaptian Klamath *lak* 'hair'
Kalapuyan **lonka* 'feathers'
Takelma *uuluk̓i* 'hair'
Huave *-limb* 'feathers'
Zuni *la-* 'feather', *lata-* 'wing', *laˀhi-* 'fly'
Atakapan Calcasieu *naw* 'feathers, wool', *nuk* 'wing'
Tunica *-alakaʃi* 'feathers'
Zapotecan Chatino *lkaˀ* 'feather'
Zaparan Zaparo *nakeso*, Iquito *panakaci* 'hair'
Mapuche *lonko* 'hair, head'

'big, long, far'

Interior Salishan Kalispel *wis* 'far'
Chemakuan Quileute *wiiluu* 'far'
Sahaptian Sahaptin *wiyat* 'far'
Yokutsan **waˀ* 'big'
Mayan Tojolabal *niwan* 'big'

Utaztecan **way* 'big, old'
Siouan Osage *wehiże* 'far'
Chitimacha *waaíi* 'wide', *weći* 'far'
Yukian Yuki *wa?* 'wide', *wic* 'far'
Tarasco *wánikua* 'many', *iaunáni* 'far'
Arawakan Dominica *uairi* 'big, old'
Mataco *wuh* 'big'
Mapuche *vuta* 'big'

'drink' or 'water'

Salishan **qaw* 'drink'
Chemakuan Chemakun *quucili-* 'drink'
Wakashan **q̇aw-* 'water'
Sahaptian **?ukuun-* 'drink', **?ukuns* 'water' (Klamath *skuu-* 'swallow', Molale *sukun-* 'drink', *?ukuns* 'water', Nezperce *kuu-* 'drink', *kuus* 'water', Sahaptin *cu-* 'drink', *cuf* 'water')
Takelma *?uuk-* 'drink'
Yokutsan *?ukun-* 'drink'
Zoquean **?uk-* 'drink'
Totonacan **q̇oti* 'drink'
Mayan **?uḱ-* 'drink'
Atakapan Calcasieu *kawkaw* 'water'
Chitimacha *ku* 'water'
Tunica *kɔra* 'drink (from **kaw-*)
Natchez *kuɴ* 'water'
Muskogean **?uki* 'water'
Yukian Yuki *?uk* 'water', Wappo *?uk-* 'drink'
Pomoan **xo-* 'drink'
Coahuilteco *ouxo* 'drink'
Timuqua *uki* 'rain'
Zapotecan Chatino *ko?o* 'water'
Beᶜoya *oko* 'water', *unkusi*, *kono* 'drink'
Uitoto *hagonoi* 'water', *hiroko* 'drink'
Tupian Cocama *kurata* 'drink'
Tehuelchean Tehuelche *komaki* 'drink', Haus-Chon *kon* 'water'
Alakaluf *acakwar* 'drink, water'

Hokan-Coahuiltecan forms suggest that there may have been a relationship between **axa* 'water' and **axa-w* 'drink', the latter then being subject to vowel assimilation resulting in **uxu*. Such a relationship, applied to the comparison of words for 'water' and 'drink' in the New World make it possible to connect many other forms than those cited above.

'eat'

Sahaptian Sahaptin *tkwata-*
Yokutsan *xata-*
Zoquean **kay-*, Zoque *ku?t-*
Mayan **q̇uʃ-* (Pokonchi *kuʃoh*, Chontal *kuʃe?*)
Utaztecan **kʷa-*
Chitimacha *ḱuʃt-*
Pomoan *kaw-*
Uitoto *gui*
Tehuelchean *katam*

'earth'

Coast Salishan Nooksack *mixʷ*
Interior Salishan *mul*
Mayan Chicomuceltec *?amab*
Chontal *?amaɕ'*
Seri *?amt*
Yuman **?amaṭa*
Yukian Wappo *?oma*, Yuki *?on*
Siouan **amá* (Dakota *makha*, Osage *monzon*, Catawba *mona*, Ofo *aman*
Arawakan Dominica *múa*
Yagua *muka*
Mapuche *mapu*
Paonoan Caxinaua *may*

'not'

Coast Salishan Chehalis *miłtans*
Siuslaw *ḱmnc*
Maiduan **men*
Mayan **miní?* (Lancandon *miní?*, Tzotzil *mu*, Tojolabal *mi*, Tzeltal-Yucatec *ma?*)
Arawakan **ma-*
Quechua *manan*
Panoan Caxinaua *rama*
Tupian Guarani *tema*
Mapuche *mika*
Tehuelchean Haus *kma*

At least two short elements, *n* for the first personal pronoun and *m* for the second, can be safely added to the list of recurrent elements even tho they do not meet the CVC requirement. The point is that they are so numerous as to virtually eliminate the chance factor despite their brevity. In fact, even if one disregarded the cases which have one or the other and included only the lan-

guages which have both *n* and *m* for first and second person respectively, and if one holds to the restriction that both forms must belong to the same functional type—whether independent pronoun or subject, object or possessive affixes—the list of language groups would still be fairly impressive. It would include families of the Penutian and Hokan-Coahuiltecan phyla, Aztectanoan, Chibchan, and Mapuche. In parts of Aztectanoan and Chibchan, and in Arawakan, second person *m* gives way to *p* or *b*, and it is at least possible that the bilabial stop may be somehow derived from the nasal. In much of South America, first person *n* is replaced by *y*; in certain areas one finds *ñ*. The palatalized *n* may be a transition form, which could have easily arisen as the result of a preceding front vowel. That is, *ʔina* could give *ʔiña*, and the latter could have developed into *ʔiya*. In fact, forms approximating all stages of this transition can be attested, thus Sahaptin *ʔin*, Esselen *ʔene*, Yuman *ña*, Chontal *ʔiya*. If we can thus derive first person *y* from *n*, then the *n*–*m* pronominal set extends from Chinook (*naika* 'I', *maika* 'thou') in northwestern United States to Ona (*ya* 'I', *ma* 'thou') on the Straits of Magellan.

2. The Problem of Proof. Given a small collection of likely-looking cognates, how can one definitely determine whether they are really the residue of common origin and not the workings of pure chance or some other factor? This is a crucial problem of long-range comparative linguistics. If we can develop dependable tests of historical causation which can be applied to small quantities of evidence, we can perhaps hope to penetrate 15,000 years of prehistory. Without such tests, we may be at a loss even at half that range.

These remarks apply not only to a mere handful of cognate sets, like the dozen or so we have presented above, but also to far more extensive and well analyzed demonstrations. A classic instance is Sapir's case for the Hokanoid grouping, a theory fully accepted by many, accepted with reservations by some and treated by still others as tho it were sheer speculation. Hoijer for example says:[6]

> It must be remembered, however, that all of these later extensions of the Hokan stock were made on the basis of scanty and, in some cases, poorly recorded materials. Before it can definitely be stated that there is a Hokan stock—even in the limited sense in which it was originally set up by Dixon and Kroeber—a good deal of work remains to be done. The evidences presented so far are far from conclusive and can serve only as a basis for future work.

And elsewhere the same author says:[7]

> It is evident from this brief survey of Sapir's work that he achieved his revisions of the Powell classification by the strict application of the comparative method to American Indian materials. Because these materials, in many cases, were fragmentary or otherwise unsatisfactory, his formulations lack completeness and, at least to some of his critics, validity.

[6] Harry Hoijer, Introduction, *Linguistic Structures of Native America, Viking Fund Publications in Anthropology* 6.17 (1946).

[7] Methods in the Classification of American Indian Languages, in Leslie Spier, ed., *Language, Culture and Personality* 8 (Menasha 1941).

In these last phrases, it is indicated that validity has been up to now a subjective matter. Hoijer evidently holds that for some critics 'lack of completeness' is tantamount to non-validity. He does not discuss whether this criterion is scientifically justified. To the present author it seems to be a non-sequitur, like saying that Old Prussian is not a Baltic language because 'it is known to us only from a few written documents of the fifteenth and sixteenth centuries. . . .'[8] Hoijer himself, his remarks indicate, recognizes Sapir's method as sound but holds that he has not gone beyond a good prima facie case. He does not state just how much evidence is required to establish a linguistic relationship. A similar lack of explicitness is reflected by Rowe, when he states[9] that '. . . our primary criterion for relationship should be lexical similarity and our proof, the establishment of numerous and far-reaching systematic sound correspondences.' Thus he claims that 'numerous' correspondences are required but he does not tell how numerous they must be.

The solution to the present situation, which may be characterized as involving impressionistic criteria as to what is or is not proved, is to work out a mathematical basis of proof. In other words, we need a methodology by which we can determine that such and such a number of cognates of such and such type are needed to prove relationship within a statable degree of confidence. The proffered proof can then be checked by the mathematical formula, and judged to be of such and such a standard of reliability. The author has already experimented a bit along these lines,[10] but much more remains to be done. The present paper illustrates and discusses some improvements over his previous efforts.

In order to prove common origin of languages, it is necessary to show that the similarities found cannot be due to other than genetic factors. The non-genetic factors include chance, universal tendencies of sound imitation and sound symbolism, borrowing, excessive inventiveness on the part of the comparativist in devising convenient phonologic formulations to make compared elements correspond and in assuming semantic shifts which will add to the number of assumed cognates.

A simple way to reduce the sound-imitative factor to a negligible minimum is to omit from consideration all such words as 'blow, breathe, suck, laugh' and the like, that is all words which are known to lean toward sound imitation.

The borrowing factor can be held down to a very small percentage by sticking to non-cultural words. It is possible to observe the extensiveness of loan intrusions in historical cases and thereby measure the weight of this factor. The ninety-seven item test list used in the present paper has so far never been found

[8] Knowledge of Old Prussian is described in these words by Leonard Bloomfield, *Language* 60 (New York 1941).

[9] John Howland Rowe, *Linguistic Classification Problems in South America, University of California Publications in Linguistics* 10.23 (1954).

[10] Mosan I: A Problem in Remote Common Origin, *IJAL* 19.43–44 (1953). This is not the first effort at a mathematical proof of relatedness, credit for which evidently belongs to B. Collinder, La parenté linguistique et le calcul des probabilités, *Uppsala Universitets Årsskrift*, 1948. Collinder's approach is based on structural elements rather than vocabulary.

to contain more than two historical borrowings, even in languages which have been under strong and constant Spanish influence for four centuries. In most cases there is not even one borrowing among the 97 items.

A further safeguard with reference to loans rests in the fact that recent loans can often be recognized by the phonetic form and by details of usage. One also has the facts of time and place—for example, it is out of the question to consider Maidu *sim* 'mouth' a borrowing from Quechua *simi*, because of the distances involved. The possibility of ancient borrowing has to be considered. Various criteria can be adduced at times—phonetic form, the distribution of the word (does it occur in a continuous area and among unrelated languages), the presence of synonyms, possibilities of a cultural basis for the borrowing—to determine whether a loan may have occurred. Any clearly identifiable loans should be eliminated from the test. In case of doubt, percentages may be calculated with and without the doubtful words.

In judging as between cognates or common loan forms, one must avoid the categoric assumption that all close similarities are due to borrowing. Even very divergent cognate languages are likely to have a few true cognates which appear to be 'ringers', for example Russian *nos* 'nose' or *t'ři* 'three'. Also the effects of sound changes need not be devastating. Cognates in English and Gaelic are generally far removed from each other because of the extensive phonological modifications which have taken place in each branch since the common Indo-European period. By contrast, Modern Greek and Spanish often agree perfectly in their consonants. In consequence of such variation in phonetic development, the only reliable approach is to seek evidence as to precisely what phonetic changes have taken place in the prehistory of each language and to judge word relationships insofar as possible in the light of known phonological correspondences. Until the phonology is worked out, one has to make tentative use of reasonable working hypotheses.

Let us suppose that a comparativist has set up one or more erroneous formulae of sound correspondence. Can this so increase the number of apparent cognates as to make a strong case for common origin between two languages which are unrelated? Can a clever comparativist build up a purely imaginary relationship into the semblance of proof? This may conceivably be possible if the only test of relationship is impressionistic, but with an objective measure of chance the number of phonologic assumptions enters as a factor in the calculation and the virtuosity of the comparativist is canceled out.

There are also ways of controlling the factor contributed by the semantics of comparison. One method, which is feasible for time depths within certain limits, is to count only exact equivalences. The word for 'water' in language A is compared, for statistical purposes, only with the word for 'water' in language B. Any similarities with the word for 'rain' or 'river' or 'drink' are disregarded in the calculation. A second approach, which can be applied when it is felt necessary to bring different meanings together, is to do so within stated limits and the effect of this taken into account. Thus, if 'water' in language A can be compared either with 'water' or 'drink' in language B, this doubles the chances of finding cognates, and by the time 'drink' of language A is compared with both in B, the

chances are quadrupled. Comparisons made only because of the eagerness of the comparativist to prove his case thus are canceled out in the final analysis of proof.

In these ways, then, one can reduce to negligible proportions the factors of sound imitation and borrowing, and one can take into account the quantitative effect of different phonologic and semantic assumptions. This reduces the problem to a choice between the factor of pure chance and the effect of common origin. A mathematical index of pure chance is derived. If the actual comparisons exceed this index by a safe margin, then an inference of common origin can be confidently drawn.

3. Two-Language Agreements, Zoque–English vs. Zoque–Totonac. MacQuown in 1942 suggested the possibility of common origin among the Totonacan and Zoquean families and the Mayan stock.[11] Wonderly examined the matter and concluded that the evidence was insufficient to eliminate the alternative explanations of chance or borrowing.[12] To support his conclusion Wonderly cited the number of similar words he had found between Zoque and Totonac, about 200, and stated that he found approximately as many between Zoque and English. His procedure, of using English-Zoque as a measure of chance similarity, represents a laudable effort to get away from the subjective judgment of what quantity of apparent cognates could or could not result from chance, but it lacks exactness nevertheless because it does not define what constitutes a similarity and it does not set a limit as to the number and type of vocabulary elements among which similarities are sought (Wonderly of course had a larger vocabulary of English at his disposal than Totonac).

Let us repeat Wonderly's experiment, using the 97-word test list that has already been mentioned. The comparison is shown in Table 1, which gives Zoque between English and Totonac. Also included are Yucatec of the Mayan stock, and Huave, which will further serve to illustrate the general problem.

Among the 97 words listed, we find the following which might be called similarities between Zoque and English:

pak	bone (older, *baan*, **bain*)	*toɕ*	stab
pik	feather (from older **petr-*)	*tenay-*	stand
kiˀ	hand (from **kond-*)	*hem-*	swim
hemeɕ	heavy	*teˀa*	that
ˀih	I (older **ih*, *ik*, *eg*)	*toɕ*	tongue
kiniˀ	neck	*tiɕ*	tooth
kono	short (from **skort-*)	*yomo*	woman
hokoh	smoke		

[11] Norman A. McQuown, Una posible síntesis lingüística macromayance, *Mayas y Olmecas* 37–38 (Mexico 1942).

[12] William L. Wonderly, Sobre la propuesta filiación linguística de la familia totonaca con las familias zoqueana y mayance, *Ponencia ofrecida para la V Reunión de Mes Redonda de la Sociedad Mexicana de Antropologia*, julio de 1951. Dr. Wonderly was kind enough to let me see a copy of his paper and also to supply a Zoque vocabulary for this study, given in Table 1. The Totonac vocabulary was taken from Pedro Aschmann, Vocabulario de la Lengua Totonaca, Instituto Lingüístico de Verano, Mexico 1949, the Yucatec was kindly supplied by Alfredo Barrera Vásquez, and the Huave by Milton Warkentin.

TABLE 1. *English, Totomayan and Huave*

	ZOQUE	TOTONAC	YUCATEC	HUAVE
all	*mumu*	*paks*	*tulakal*	*meawan*
ashes	*kuy-ham*	*łka?ka?*	*ta?an*	*ngwiat*
bark	*naka*	*qu?cqa?*	*u-pac-ce?*	*o-pang*
belly	*ɬek*	*paan*	*nak*	*o-tieng*
big	*miha*	*λanka?*	*nohoc*	*na-tang*
black	*yik*	*ɬi?ɬa?qa*	*ɬ́k*	*na-mbeor*
blood	*ni?pin*	*qa?ł-ni?*	*kik*	*kieh*
bone	*pak*	*lukut*	*bak*	*o-laaɬ*
burn	*ne?m-*	*łkuy*	*elel*	*nkatitit*
cloud	*?o?na*	*puk-łni?*	*muyal*	*oik*
cold	*pakak*	*qa?wi?wi?*	*si?is*	*na-kind*
come	*min-*	*min*	*talel*	*(im)iin*
die	*ka?-*	*nii-*	*kimil*	*ndeow*
drink	*?uk*	*qu?ta*	*ukul*	*-nganeow*
ear	*kowi*	*taqaan*	*ʃikin*	*o-laag*
earth	*nas*	*ti?ya?t*	*lu?um*	*iit*
eat	*wi?k-*	*wa?-*	*hanal*	*iet*
egg	*poka*	*qaa?łwaa?t*	*he?*	*o-mb*
eye	*witim*	*laqastapu*	*wic*	*o-niiag*
fat	*ɬanga?*	*mantiika* (Span.)	*ɬaɬ*	*ɬapay*
feather	*pik*	*pa?qa?*	*ɬ́kukum*	*o-limb*
fire	*hukitik*	*łkuyat*	*ka?ak*	*biimb*
fish	*koke*	*s-kii?ti?*	*kay*	*kiet*
five	*mohsa?y*	*kiɬis*	*ho?*	*kokiaw*
fly	*sitiht*	*qusa*	*ʃiknal*	*-hlil*
foot	*ne?ŋ*	*tuwan*	*ok*	*o-leah*
give	*ɬi?-*	*maa?staa?-*	*ɬ'a*	*iic*
good	*?oye*	*λaan*	*uɬ*	*na-hneah*
green	*ɬuhɬuh*	*staqni?nki?*	*ya?aʃ*	*na-teaik*
hair	*way*	*ya?hni?*	*ɬo?oɬ*	*o-ndeaɬ*
hand	*ki?*	*makan*	*ɬ́kab*	*o-wiʃ*
head	*ko-pak*	*a?q-ʃaaqa*	*pol*	*o-mal*
hear	*matoŋ-*	*qaʃ-mata*	*u?yik*	*-ngeay*
heart	*ɬokoy*	*naku?*	*puksiikal*	*o-meeaɬ*
heavy	*hemeɬ*	*ɬinka*	*aal*	*na-im*
I	*?ih*	*kit*	*te-n*	*ʃike*
ice	*ɬatuh*	*miki*	*yelo* (Span.)	*yelo* (Span.)
know	*mus-*	*ka?ɬii-*	*ohel*	*ndiy*
leaf	*?ay*	*tu?waa?n*	*le?*	*op*
lie	*?iŋgek*	*ma?-*	*cital*	*peaaw*
liver	*pa?t*	*-ł-wa?ka?ka?*	*taman*	*ik*
long	*piy*	*łmaa?n*	*cowak*	*na-hal*
man	*pin*	*ci?ʃku?*	*ʃib*	*na-ʃey*
many	*wiwi*	*łuuwa?*	*ya?ab*	*ʃowiy*
meat	*sis*	*lii-wa?*	*bak*	*o-nih*

TABLE 1—*Continued*

	ZOQUE	TOTONAC	YUCATEC	HUAVE
mountain	*poʔmna*	*sipih*	*puʔuk*	*peat*
mouth	*ʔaynaka*	*kitniʔ*	*ci'*	*o-mbeay*
name	*niy*	*tapaa-kuwiit*	*kabaʔ*	*nit*
neck	*kini*	*piʃ-niʔ*	*kal*	*o-nik*
new	*home*	*saastiʔ*	*tumben*	*hayaɬ*
night	*ɬuʔ*	*ciʔsniʔ*	*akab*	*o-ngwiiaɬ*
nose	*kini*	*kinkan*	*niʔ*	*o-ʃing*
not	*haʔn*	*niinta?*	*maʔ*	*ngo*
one	*tumi*	*-tum*	*hun*	*nop*
person	*pin*	*ciʃkuʔ*	*winik*	*nipilan*
rain	*tuh*	*siin*	*caʔak*	*na-hoet*
red	*ɬapas*	*ɬuʔɬuʔqu*	*cak*	*na-kanɬ*
river	*niʔ*	*qaltuucuquh*	*ukum*	*lam*
road	*tuy*	*tihiʔ*	*be*	*tiiid*
root	*waɬi*	*tanqa-ʃiiqa*	*moɬ*	*o-ʃiic*
sand	*waʔna*	*kukuh*	*sus*	*wiiit*
say	*nim-*	*-wan*	*waʔalik*	*-sah*
see	*ken*	*uʔkʃila*	*wilik*	*-haw*
seed	*tim-buh*	*talɬiʔ*	*nek*	*o-saab*
short	*kono*	*aʔk-ɬuu*	*kom*	*tokoɬ*
sit	*poks-*	*wila-*	*kutal*	*ɬotoh*
skin	*naka*	*ʃuuwaʔ*	*oʼ*	*o-taag*
sleep	*ʔiy-*	*ł-tata-*	*wenel*	*-meay*
small	*ciks*	*aʔk-ɬuu*	*cican*	*kiceec*
smoke	*hokoh*	*hiniʔ*	*buɬ'*	*na-sop*
snake	*ɬan*	*luuwaʔ*	*kan*	*ndiik*
stab	*toɬ*	*ł-tukuuni-*	*lom-*	*-hɬol*
stand	*tenay-*	*kahyah*	*waʔtal*	*lomboh*
star	*maɬa*	*staʔku*	*ek*	*o-kas*
stone	*ɬaʔ*	*ciwiʃ*	*tunic*	*kang*
sun	*hama*	*ciʔciniʔ*	*kin*	*nit*
swim	*hem-*	*ʃ-kiwaatnam*	*tahal haʔ*	*-hrok*
tail	*yeʔyu*	*s-tahan*	*ne*	*o-wil*
that	*teʔa*	*aaʔmaʔh*	*leloʔ*	*ayiin*
this	*yiʔwi*	*yuʔmaʔ*	*lelaʔ*	*aag agiy*
thou	*mih*	*wiʔʃ*	*te-c*	*i-ke*
three	*tukaʔy*	*-tuʔtu*	*oʃ*	*aroh*
tongue	*toɬ*	*siiʔmaʔqaat*	*ak*	*o-niw*
tooth	*tiɬ*	*taɬan*	*ko*	*o-liek*
tree	*kuy*	*kiʔwiʔ*	*ceʔ*	*ʃiil*
two	*meɬa*	*-tuʔy*	*kaʔ*	*ih-*
walk	*wit-uʔ*	*tuwanii-ʔaʔn*	*ʃimbal*	*-hiy*
warm	*pih-*	*ciʔci*	*kiʔnal*	*neoraar*
wash	*ɬeʔ-*	*caʔqaaʔ*	*poʔ*	*-hanɬ*
water	*niʔ*	*cuʔcut*	*haʔ*	*iyow*

TABLE 1—*Continued*

	ZOQUE	TOTONAC	YUCATEC	HUAVE
what	*tiyɨ*	*ntuu*	*baʔaʃ*	*ngin*
where	*nhuta*	*nii*	*tuʔuʃ*	*niing*
white	*popo*	*s-napapa*	*sak*	*raan*
who	*ʔiwɨ*	*tii*	*maʔaʃ*	*hane*
wing	*sah*	*paqan*	*ʃik*	*o-limb*
woman	*yomo*	*puskaat*	*ċup*	*na-htah*
yellow	*puʔɸi*	*s-mukuku*	*kan*	*maril* (Span.)

One omitted comparison may be pointed out to illustrate the use of the strict semantic criterion. Zoque *ken* 'see' is compared with English 'see' and not with the archaic and dialectal *ken*, a variant of *know*. We recognize that a semantic change from 'see' to 'know' is fairly natural and that *ken*: *ken* represents a better comparison than most of those listed above. It probably was one of the 200 English-Zoque similarities found by Wonderly, but we pass it by because it does not meet the requirements of our test.

As against the 10 approximate similarities in Zoque-English, we find a number perhaps only slightly larger between Zoque and Totonac, but they include several which show simpler and more complete agreement, to wit:

come	*min-*	*min*
eat	*wiʔk-*	*waʔ-*
feather	*pik*	*paʔqaʔ* (cp. also Tot. *paqaan* 'wing')
fish	*koke*	*skiiʔti*
hear	*matoy*	*qaʃ-mata*
nose	*kini*	*kinkan*
one	*tumɨ*	*-tum*
pierce	*toɸ*	*-tukuuni-*
this	*yɨʔwi*	*yuʔmaʔ*
tooth	*tiɸ*	*taɸan*
tree	*kuy*	*kiʔwiʔ*
wash	*ɸeʔ-*	*caʔqaaʔ*

The list is not only more convincing in appearance but contains within it some first suggestions of a plausible phonologic theory. *p t m n w y* correspond exactly. Both *ɨ* and *a* of Zoque correspond to *a* of Totonac, and Z. *o u* correspond to T. *u*. Z. *k* equals T. *q*, except before *i* where T. has only *k*, and there may be other valid cases of *k* = *k*. Z. *ɸ* may correspond either to T. *ɸ* or *k*. From these relationships we can reconstruct three consonants *ɸ k q* for the presumed common period, which in most positions remain distinct in Totonac but give respectively *ɸ ɸ k* in Zoque; in addition there may have been an original rounded k^w which gives *k* in both languages but may be reflected in a rounding influence on vowels (Z. *koke* 'fish' from reduplicated *k^w*ak*^{w}*ay*, T. *kii-* from *k^w*ay*). A general search of the Totonac and Zoque vocabularies corroborates and only slightly amends these phonologic formulations. By corroboration of a phonologic theory is meant the discovery of still other elements in the compared languages whose phonemes correspond according to the formulations being tested. In the case of Zoque-

Totonac, the total number of presumptive cognates, found by the author in vocabularies of perhaps 2000 words, hardly exceeds sixty (as compared with Wonderly's 200). Some of the phonologic correspondences are represented by a number of examples, others by very few, for example only three good instances of $\phi = k$. The important thing is not so much the number of examples as their phonologic consistency. Nor need we be concerned that an examination of 20 times the vocabulary gives only six times as many cognates, since our first list of 97 items of meaning covers precisely the area of maximum conservatism.

We do not need proof of correct phonologic formulas in order to test the validity of the historic connection between the languages. All we need is concrete assumed equivalences whose statistical weight can be reckoned in the computation of the chance factor. The method of accomplishing this can now be explained and demonstrated. For the strictest calculation of the chance factor, we would need to obtain statistics on the relative frequency of each phoneme of each language or to otherwise allow for the differences. For the present purposes, a method of safe approximation will be used, that is, a calculation which errs on the side of discounting rather than emphasizing possible relationship.

Our first problem is to calculate the chance factor, granted the circumstances of the present case. The typical minimum stem form in both languages is CVC, with only demonstratives and formative elements falling below it. Some of our vocabulary elements are more complex. For the sake of uniformity, we can disregard any elements which fall below a CVC complexity and consider only the first CVC of elements which go beyond this. We need to know how many different possible distinct CVC formations may enter into our comparisons. This is determined partly by the number of consonants and vowels in each language and partly by the phonologic correspondences which are assumed to be possible.

Zoque has six vowels, *a e i o u ɨ*, and 11 independent consonants, *p t ɸ k ʔ s h m n w y*; six others, *b d g ñ c ʃ*, occur as the result of certain phonologic assimilations and contractions. Totonac has three vowels, *a i u*, and 17 consonants, *p t k q ɸ c ƛ ʔ h s ʃ l m n l w y*, some of which may interchange in response to augmentative-diminutive symbolism. Because of diminutive symbolism—e.g. *kinkan* 'nose' may be a diminutivized form derived from reduplicated **qinqan*—it is safe to count only 10 independent consonant phonemes, with an eleventh, namely glottal stop, independent except in the post-vocalic position.

In making comparisons between the two languages either *ɨ* or *a* of Zoque may correspond to Totonac *a*. The Zoque contrast between *ɨ* and *a* therefore has no significance for comparative purposes. We can state this as $ɨ + a = a$, and may call such a set of phonemes a synophon. There are two other vocalic synophons, $e + i = i$ and $o + u = u$. The consonantic synophons are ten: *p t m w y ʔ h s*, $n = n + l$, $ɸ + k = ɸ + k + q$. The number of possible CVC complexes, in terms of synophons, is therefore $10 \times 3 \times 10$, or 300.

Given an item of meaning expressed by a given CVC in one of the compared languages, there is 1 chance in 300 that the same meaning will be expressed by the same CVC in the second language. Or, at least, this would be the case if all the consonantic synophons were of approximately equal frequency and if the

same were true of the vowels. Sharp differences in frequency have the effect of increasing the chance factor. For example, if one of the synophons approached zero and the remainder therefore were each increased, the effect would be nearly the same as having one less synophon. Or if any one synophon is relatively very frequent, say with the others running at a low average, this has a similar effect. Minor fluctuations up and down about the average have little effect. In applying these considerations to our problem, we must note at once the high frequency of $i + a = a$ among the vowels. This has an important effect, since there are only 3 vocalic synophons to begin with. If we had only 2 vowels the number of CVC would be $10 \times 2 \times 10$, or only 200 instead of 300. Inequalities among the consonants, for example the high relative frequency of $t + k = t + k + q$, have a somewhat smaller effect, even tho there are two positions of consonants, because we have a larger number of consonants to begin with. With 9 instead of 10 consonants and 3 vowels, we would have $9 \times 3 \times 9$ CVC possibilities, or 243 instead of 300. It should be safe to assume that the effect of unequal frequencies on our index will not be greater than two-thirds, or that the corrected index would not be smaller than that resulting from 7 consonants instead of 10, and 2 vowels instead of 3, that is 1 in $7 \times 2 \times 2 = 98$ or approximately 1 in 100.

It would be helpful to have an approximate standard index to apply to any pair of languages without the necessity of going thru the analysis of synophons in each case. We could then apply this to Zoque-English in the immediate problem and to various other cases which we have yet to consider in this paper. It is suggested that 1/100 may be a convenient and satisfactory index for such a purpose, provided that the choice of phonologic correspondences is made with circumspection and in realistic fashion and if the amount of apparent phonologic change does not appear unusually large. If there is any reason for uncertainty or if the results are seriously challenged, the only recourse is to actually determine the number of synophons or, better yet, to work out an index based on phoneme frequencies.

We can give our main attention to CVC complexes, as a generally usable and convenient standard. Occasionally one can use more complex forms, say CVCV or CVCC or CVCVC, giving successively smaller phonetic ratios and thereby making the elimination of chance easier. Sometimes we have to deal with smaller complexes, say CC (a sequence of two consonants, with vowels absent or too variable to count) or CV. The ratio in the first case is 1/49, in the second 1/14. Less than CV is likely to be completely impractical, since the ratio for single consonants would be 1/7, unless the number of synophons were much higher than the standard we have set.

After we know the working phonetic ratio (one over the total number of complexes possible, with allowance being made for variant frequencies and a margin of safety), it is necessary also to take into account the fluctuations of chance. That is, even tho a given form represents one phonetic possibility out of 100, we will not get exactly one accidental resemblance in every 100 pairs of items compared. Instead there will sometimes be, by accident, less than 1 agreement, and sometimes more than one. A standard formula for calculating the

chances of getting a given number of coincidences may be translated for our purposes into the following form:[13]

$$P_c = C_{n,c}(r)^c(1-r)^{n-c}$$

This means that the probability of c correspondences in a list of n meanings is the number of different possible combinations of c correspondences among n items multiplied by the phonetic ratio raised to the power of c and by the ratio of phonetic non-agreement (unity minus r) raised to the power of the number of non-agreements $(n-c)$. In our CVC test, $n = 97$, $r = 1/100$, $1-r = 99/100$, $c =$ the number of CVC correspondences found. Solving the equation for one to five correspondences:

Correspondences	*Probability pct.*	*Appr. fraction*
1	36.8	1/3
2	17.8	1/6
3	5.8	1/17
4	1.4	1/71
5	0.27	1/370

And the chance probability gets rapidly smaller as one increases the number of CVC agreements.

Five percent is usually considered a safe limit between the uncertain and the scientifically reliable. Therefore in any instance where the 1/100 phonetic ratio is on the safe side, 4 CVC agreements give a sound basis for recognizing historic connections between two languages. Three of them are almost satisfactory, but less than that are insufficient in themselves to give confidence of genetic connection.

Applying these criteria to Zoque-English, we find only one acceptable CVC agreement, namely 'tongue', and this fails to establish a connection. The number of good CC agreements ('tongue', 'stand') or CV ('tongue', 'bone', 'short') is in each case too small to suggest common origin. By contrast, Zoque and Totonac have eight or ten good CVC correspondences—'come, eat, feather, hear, nose, one, pierce, tooth, fish, tree'. The counting of the last two depends on the recognition of a secondary reduction (*koke* from **k^{w}ak^{w}ay*) and a transposition (Z. *kuy* from **kiw*, or T. *ki?wi?* from **qu?yi?*). Even counting only eight agreements, the probability of its being pure chance is only 1 in 100,000, and historic connectedness is an extremely safe conclusion.

4. Use of Archaic Forms, Totonac-Yucatec. The soundest comparisons are those based upon the earliest known or reconstructible forms in each language. This can be obtained by means of historical data or with the aid of internal and comparative evidence. Occasionally the result is to eliminate a sound-appearing CVC agreement when it is found that one or both of the forms is secondary.

[13] The author has been helped with mathematical aspects of these problems by Louis Cote of the Statistics Laboratory of Columbia University and by Leonard Lerman of the University of Colorado. In calculating the CVC chance probabilities, Prof. Lerman explains they were 'calculated as a Poisson distribution with a mean of 0.97.'

Often the effect is to reveal CVC relations that have been obscured by recent phonological or structural changes.

Totonac *ɬu'ɬu'qu'* appears to be cognate with Yucatec *cak*, both meaning 'red', but at first blush they do not seem to involve CVC agreement. Cognate forms to the Yucatec within the Mayan stock include Aguacatec *caq*, Tzeltal *ɬah*, Tzotzil *ɬoh*, Chicomuceltec *cakuy*, leading to a reconstruction **ɬaqu*. Therefore, if one disregards the first vowel, Totonac and Yucatec have in common CCV, a form of exactly equal complexity with CVC and therefore of equal weight in its relation to chance. From internal evidence in Totonac we can reassure ourselves that the first vowel does not stand in contradiction to the relationship with Mayan. Totonac has *smukuku* 'yellow', *snapapa* 'white', *ɬi'ɬa'qa* and *ɬi'ɬi'q* 'black' (the latter form in the Zapotitlan dialect). Such words exhibit a Totonac tendency to vocalic harmony, and suggest that the present Totonac 'red' may well go back to a form like our Mayan reconstruction. The glottal stops may be due to a symbolic process, as seen in *pa'qa'* 'feather', *paqan* 'wing'.

Mayan words for 'fish'—Yucatec *kay*, Pokomchi *kar*, Tzeltal *cay*, Tzotzil *coy*—suggest a protoform **k*ʷ*ay*, agreeing with the form required in comparing Totonac with Zoque. Huave *kie-t* also seems to fit.

Yucatec *wa'alik* 'say' can go back to a CVC cognate with Totonac *-wan* (also cf. in the Zoquean family North Mixe *wamp* 'say', Zoque *wan-* 'sing') if it is a dissimilation of an older **wan'alik*. As yet the only supporting evidence in Mayan is the occurrence of *-al* and *yal* in other languages with the meaning 'say'; the Yucatec may be a compound of *wan-* with this element. Perhaps new evidence may eventually be found to support this reconstruction.

There being no other CVC agreement between Totonac and Yucatec in the test list, we end up with two acceptable ones and another possible. These are not sufficient in themselves to meet the criterion for proving common origin. At the same time it may be noted that these agreements exceed what we found in the English-Zoque comparison. The evidence is thus strongly suggestive even tho inconclusive.

5. Chain Relationships, Totomayan. When languages have so far diverged that it is difficult or impossible to prove their relationships directly, it may still be possible to do so by a chain procedure, which takes into account certain important phenomena of dialect relationships. Unless a speech community is abruptly separated, either by the migration of a part of it or by foreign encroachment across its area, the language tends to differentiate gradually into local dialects in such a way that neighboring groups speak more nearly alike than those at a distance. In the typical situation of three dialects located in the order ABC, A and C will be linguistically as well as geographically farther apart than A from B or B from C. Languages which have developed out of such a situation will show chain relationships.[14]

[14] Cf. discussion by Emile Benveniste (La classification des langues, *Conférences de l'Institut de linguistique de l'Université de Paris*, 11.38, 1952–53) of 'connexions de caractère sérial' or 'parentés par enchaînement'.

The case we have been discussing evidently illustrates this phenomenon. We have already seen proof of relationship between Totonac and Zoque. Between the latter and Yucatec we find five unquestionable CVC agreements—'bone, drink, eye, neck, snake'—and three more which are nearly satisfactory—'black, give, short'. This too constitutes satisfactory evidence of common origin. If Totonac has a common origin with Zoque, and Zoque with Yucatec, then Totonac-Zoque-Yucatec are present-day representatives of an old chain of mutually intelligible dialects. The two CVC agreements found between Totonac and Yucatec confirm this finding, even tho by themselves they are insufficient to fully establish genetic relationship.

6. Proof by Intersection, Huave–Totomayan. Another way of resolving an indecisive proof of relationship is by calculating the convergence of chance as between one language and two or more others. This method can be illustrated with Huave taken in conjunction with Totonac, Zoque and Yucatec.

Huave has evidently undergone a complex development in its vowels, including modifications in response to adjoining consonants and diphthongization in the direction of the vowel of the following syllable. The latter can even be illustrated in recent additions to the vocabulary, such as *meac* from Spanish *mecha* 'wick'. Other changes seem to be the loss of semiconsonants *y* and *w* between vowels, delabialization of $*k^w$ before back vowels (including *i*), spirantization of final *k*. The phonology will have to be patiently worked out from internal evidence and comparison with such distant congeners as Totomayan and Penutian; unfortunately there are no close sister languages. Operating with a very tentative conception of Huave historical phonology, it seems reasonable to suggest as CVC correspondences between Huave and Totonac 'big, come, fish'; for Huave–Zoque 'fish, come'; for Huave–Yucatec 'blood, fish', and possibly 'back'. Huave–Totonac 'where' also seems to be a sound cognate, but not necessarily CVC.

Now the chances of finding three CVC cognates between two languages is about 6%, and the chances for two agreements are 18%. The probability of getting 3, 2 and 2 coincidences between one language and the members of another set of three languages would be the product of the three percentages tripled, amounting to less than 1%. We may therefore confidently conclude that Huave is related to the three other languages. It is perhaps premature to decide whether all four should be set up as a phylum, to be called perhaps Huavemayan, or to keep Huave separate from Totomayan. Further comparison of Huave and Totomayan with Penutian is necessary in order to give the proper perspective on this problem.

7. Hokan and Hokogian. The use of CVC tests, sometimes in chain fashion and with occasional recourse to the device of intersection, is all one needs not only to demonstrate Sapir's Hokan-Coahuiltecan theory but also to show that this grouping extends farther in stepwise fashion to Jicaque in Honduras, to Yukian in northern California, and to Atakapan, Chitimacha, Tunica, Natchez and Muskogian. Because it extends from Hokan in the west to Muskogian in the

east, this phylum may conveniently be called Hokogian. Detailed proof, along the lines already illustrated with Huave and Totomayan, has been assembled in a separate paper, now in an advanced stage of preparation.

An interesting fact about Hokogian is that, while the stepwise relationships are generally speaking in rough correlation with the relative geography of the component languages, families and stocks, there is at least one important exception. This is the case of Yukian, which does not tie in closely with immediately neighboring Hokan languages but is most closely linked with southern Hokan languages, with Coahuiltecan, and with Chitimacha. From this relationship, we can infer that the present location of Yukian may be due to a relatively recent migration from an area where it was in contact with its nearest linguistic congeners.[15]

8. Reconstruction-Based Tests, Aztectanoan and Macro Chibchan. When it is impossible to obtain a satisfactory test of relationship by the direct comparison of languages and when it is complicated to do so by the method of intersection, one may be able to extend the possibility of proof by using reconstructed vocabulary for one or more terms of the comparison. This approach has been previously used for the Mosan phylum,[16] and will now be briefly illustrated by data which link several linguistic stocks in North, Central and South America. The notion that Utaztecan and Chibchan might be connected were first suggested to the author by comments on parallelism between Chibchan languages and Nahuatl in Nils M. Holmer's Critical and Comparative Grammar of the Cuna Language.[17] Subsequently he was struck by scattered resemblances between Caxinaua[18] of the Panoan stock with both Chibchan and Utaztecan.

Comparison of single languages, such as Cuna with Nahua or Hopi, failed to give decisive results. Therefore a list of comparative Utaztecan was drawn up, and compared with Chibcha[19] and with Caxinaua. The results are distinctly positive.

The reconstructed Utaztecan vocabulary, not given here because of space limitations, consists of all items of the test list for which cognate equivalents could be found in two or more Utaztecan languages, provided the two were not close sister languages representing a limited segment of the stock. In a few cases, where two proto forms seemed to have equal claim to antiquity, both were set

[15] Robert Melton finds ethnologic confirmation of such an origin, and speaks (letter of June 8, 1954) of 'ethnological evidence that the Yuki do not "belong" in California, and have had seemingly direct contact with the Pueblo and SW, most likely in passage from near New Orleans. The chiefship pattern among the Yuki is very un-Californian and very possibly closer to the SE than elsewhere. They were the most warlike in California, except for the extreme South. They sound re this much like . . . Chitimacha.'

[16] Mosan I, *loc. cit.*

[17] *Etnologiska Studier* 14 (1947) passim. Altho Holmer suggests parallels with various Amerindian languages, the Utaztecan ones, mainly Nahuatl, are more numerous and often, in the opinion of the present author, more striking than most of the other non-Chibchan instances.

[18] C. de Abreu, *Gramática, Textos e Vocabulario Cazinauás*, Rio de Janeiro 1914.

[19] Amerindian Noncultural Vocabularies 004, based on E. Uricoechea, *Gramática, vocabulario, catecismo i confesionario de la lengua chibcha*, Paris 1871.

down. It was possible to provide such reconstructed early Utaztecan forms for 69 of the test items. Alongside them were set in parallel columns the corresponding contemporary (or recent past) forms from Tewa of the Kiotanoan stock, Chibcha of the Chibchan stock, and Caxinaua of the Panoan family.

Reconstructed Proto Utaztecan and 18th century Chibcha give five and possibly seven good CVC agreements:

foot	*keʃ*	*kihca*
hair	*ɸoni*	*ɸie* (**ɸine*)
neck	*kuhca*	*gikihca*
sleep	*te-kʷepa*	*kib-*
tree	*kʷawi*	*kie*
die	*muhka*	*bgi* (**miki*)
sun	*tawa*	*sua* (**tua*)

In making comparisons between Utaztecan and Caxinaua, there were only 62 pairs altogether, because 7 of the words were not available for Caxinaua. Four acceptable CVC correspondences were found:

belly	*puʃ*	*posto*
fish	*pa-kewu*	*baka*
foot	*keʃ*	*kiʃi*
road	*pohe*	*bay*

Direct agreements between Caxinaua and Chibcha included a number of CV complexity and two CVC:

black	*miʃi*	*mihsio*
foot	*kiʃi*	*kihca*

It is interesting that Tewa-Utaztecan produced not a single CVC agreement, evidently due to recent phonological and structural reductions in Kiotanoan, but there are 12 good CV agreements in 69 pairs, sufficient to prove relationship. Direct comparison between Tewa and the two South American languages did not give an especially impressive number of agreements—five with Chibcha, six with Caxinaua. Therefore the connection rests on chain relationship with Utaztecan as the connecting link.

A Macro Chibchan phylum has been proposed in South America,[20] and the evidence so far as known to the author appears to be sound. Nevertheless, the degrees of relationship may in some cases be quite remote, so much so that Utaztecan quite possibly is as close to Chibchan as some of the other members proposed for the phylum. On the other hand, Utaztecan appears to be about as close to Chibchan as to Kiotanoan. What appears to be shaping up is a merging of Aztectanoan with Macro Chibchan in a chain relationship, whose time depth may or may not be greater than that of Hokogian. The affiliation of Panoan, which apparently has not previously been linked with Macro Chibchan, suggests that the full embrace of Kiochibchan has yet to be established.

[20] See J. Alden Mason, The Languages of South American Indians, *Handbook of South American Indians* 6.175 ff. (1950). The classification of Macro Chibchan languages is mainly the work of Paul Rivet and Jacinto Jijón y Caamaño.

9. DIVERGENCE SCALE. We have had to devote considerable attention to the problem of proof as it relates to distantly connected languages. Of equal importance is the problem of determining the order of relationship of languages and language groupings, that is the question of determining which separations took place earlier and which took place later. A constantly interconnected problem is that of degrees of separation, since dialects which remain in contact with each other tend to develop in similar directions to a greater or lesser extent, depending upon the amount of intercommunication continuing among them. Distinct neighboring languages, too, may influence each other, and it is important to understand the ways in which this may take place without confusing them with the cohesive tendency among mutually intelligible dialects.

The various criteria for determining the order of linguistic relationships, phonologic, structural, semantic, lexical, each has its advantages and limitations, and all should be used together. Only one of them lends itself to a simple mathematical scaling of similarity and divergence, that is the lexical, about which can be built a methodology of lexicostatistics. The basis of this procedure is the counting of cognates in a given test vocabulary taken for two or more related languages. Experimentation shows that this method gives apparently consistent results approximately correlated with the actual divergence. Thus it becomes possible to obtain estimates of the time depth of any linguistic differentiation, qualified by the factor of degrees of contact. The result of a lexicostatistic calculation is therefore not necessarily an actual divergence time, but an *indicated* divergence time, representing both the duration and degree of separation. Such results have to be correlated with geography and other factors in order to know their significance with reference to prehistory.

Divergence time or indicated divergence time can be stated in approximate number of centuries. In handling broad linguistic perspectives, it is often desirable to be able to refer to degrees of divergence by inclusive terms. We already have in usage the expressions dialect, language, family, stock, phylum, which some scholars, following the recommendation of Whorf and Trager,[21] try to use in consistent fashion following the order given. The author has advocated that the terms be given definite values in terms of approximate time-depths, and now proposes a modification and extension of the system, as given below in terms of estimated time-depth and cognate percent (according to the Lees-Swadesh constant). It will be necessary some day to add terms for even more remote connections.

TERM	DIVERGENCE CENTURIES	COGNATE PERCENT	GENERAL CHARACTERISTIC	EUROPEAN EXAMPLE
language	0–5	100–81	mutual intelligibility of local forms (dialects)	English
family	5–25	81–36	relationship apparent even to layman	Germanic
stock	25–50	36–12	relation obvious to comparativist	Indo-European
microphylum	50–75	12–4	rare but striking basic agreements	?
mesophylum	75–100	4–1	very rare agreements	?
macrophylum	over 100	less than 1	discoverable thru reconstruction	?

[21] B. L. Whorf and G. L. Trager, The Relationship of Uto-Aztecan and Tanoan, *American Anthropologist* 39.610 ftn. 5 (1937).

Applying such a terminology to America it is possible to correct a great many misapprehensions which are now prevalent. For example, the linguistic map of North America by Murdock and associates, sets Teton, Yankton, Yanktonai and Assiniboin on a par with Atakapa, Wakashan and Salish. But Teton is a dialect of Dakota mutually intelligible with Yankton, Yanktonai and Assiniboin; Atakapan is a family, consisting of the three languages, Hiyekiti, Calcasieu and Akokisa; Wakashan is a stock, made up of the Nootkan and Kwakiutlan families, the one consisting of Makah and Nitinat and Nootka, the other of Kwakiutl and Bellabella; and Salishan is a large and complicated microphylum.

In Sapir's classification of North American languages, there are six major groupings, not all on an equal level of time depth. Eskimo-Aleutian is a stock. Nadene is probably a stock. Algonkin-Mosan covers Mosan, which is a mesophylum, and Algonkin-Ritwan, which may be a microphylum, and Kutenai, an isolated language; the relationship among these three divisions remains to be demonstrated. Penutian in Sapir's broad sense may be a mesophylum; it includes probably two microphyla, California Penutian and West Oregonian (Alsea, Siuslaw, Kalapuya-Takelma), the Sahaptian and perhaps the Tsimesyan stocks, and the isolated language Chinook with local dialects. The inclusion of Tsimesyan requires further study, and it is possible that Zuni also belongs to a total group which would include Huave and Totomayan along with Sapir's original grouping. The scale of the enlarged group has yet to be determined; presumably it is either a meso or a macrophylum. Aztectanoan, if it is to be regarded apart from Chibchan and other South American languages, is probably a microphylum. Hokan-Siouan may be a macrophylum, containing within it, as its broadest single component, Hokogian, which is probably a mesophylum; some parts of the Hokan-Siouan hypothesis have not yet been tested.

10. AFFINITIES OF QUECHUA. In 1943 Harrington published an article, called "Hokan Discovered in South America."[22] Beyond the title he does not claim that Quechua is a Hokan language but rather that it is related to Hokan. Some of the comparisons look very sound, but the number and quality is not such as to lead necessarily to the conclusion of a special affinity between Quechua or the Quechumaran stock and Hokan. They may simply represent the kind of agreements which were discussed and illustrated above as possibly harking back to the earliest linguistic connections in the New World. Typical of Harrington's list are cases like Quechua *maki* 'hand', Pomo *ma-* 'with the hand', Salinan *meen* 'hand', and Yana *moo-* 'reach'. These are good CV correspondences but one can do much better, as it happens, outside of Hokan, e.g. Totonac *makan*, Caxinaua *mikin* 'hand', Utaztecan **mahka* 'give', **mawi* 'hand'. There are a few first-rate CVC comparisons in Harrington's material: Quechua *tawa*, Yana *tawmi-* 'four'; Quechua *muqu*, Pomo *moko* 'knee'; Quechua *para* 'rain', Yana *parii-* 'to rain'; Quechua *chiƛu*, Chimariko *cele?i* 'black'. Quechua *tiya-* 'sit', which Harrington compares with Subtiaba *-taau*, can be compared even better with *tey-* 'sit' in Chitimacha, also a Hokogian language.

Evidently no one has thought of seeking relatives of Quechumaran in South

[22] John P. Harrington, Hokan discovered in South America, *Journal of the Washington Academy of Sciences* 33.334–344 (1943).

America. Without making a thoro search but examining those few vocabularies which have come to hand the author finds evidence for relationship at least with Tupian and with Puelche.[22] With regard to Tupian, there are at least three good CVC correspondences with Cocama in a list covering only 53 of the test words and three with Guarani in an almost complete list, along with a number of other apparent cognates in both languages.

water	*unu*	*uni*	*ʔi* (perhaps **ʔini*)
cold	*ciri*	*ɬiri-ahi*	*ʃoisa*
five	*pisqa*	*piʃtak*	(*petei po* 'one hand')
man	*qhari*	(*yaisara*)	*karai*
root	*saphi*	——	*ʃapo*
small	*huc̆uy, -ci*	——	*cuʔo, mici*
big	*hatun*	*tua*	*turica*
two	*iskay*	*mukuyka*	*mokoi*
not	*mana*	*tema*	(*ʔani*)
what	*ima*	*mar-tipa*	*mbaʔe*
where	*may-pi*	*maka-tipa*	*moo-pa*
woman	*warmi*	*wayna*	(*kuña*)
this	*kay*	——	*ko*
meat	*ayca*	*ɬu*	*ʃoʔo*
eat	*mikhu-*	*mahun*	*akaru*

The comparison for 'eat' is further illuminated by Aymara *manqhu-*.

Correspondences between Quechua and Puelche in a 65 item list include at least four CVC or equivalent forms, shown here with other possible cognates:

fish	*cʔañwa*	*cale, cel*
neck	*kunka*	*akʷal*
many	*askha*	*acaha*
foot	*caki*	*ack*
heart	*sunkʔu*	*acakʷa*
warm	*qʔuñi*	*kale, kele*
I	*ñokʔa*	*kʷa*
thou	*kʔan* (**kʔama*)	*kemaw*
night	*tuta*	*ɬuman*
salt	*kaci*	*keɬan*
mother	*mama*	*mama-ka*

The last two items are not included in the relationship test list, but are part of the lexicostatistic list. The items available for the latter were 98. If the above listed comparisons may be all counted as cognates they constitute about 11% of the lexicostatistic test, indicating a relationship on the microphylum level.

[22] Quechua from A. Lira, *Diccionario Kkechuwa-español*, Universidad de Tucumán, Instituto de Historia Lingüística y Folklore 12 (1944); Cocama from P. Rivet, Les langues guaranies du Haut-Amazone, *Journal de la Société des Américanistes de Paris* 7.149–78 (1910); Puelche from Juan Federico Hunziker, Vocabulario y Fraseario Genakenn (Puelche), *Revista del Museo de La Plata* 31.261–97 (1928).

Finally confirming the reality of the relationship are the following agreements between Puelche and Guarani:

bark	*ampelc*	*pire*
black	*ikema*	*kemba*
head	*aka*	*aka*
eye	*atetk*	*tesa*
name	*maktena*	*tera*
leaf	*aɬekɬek*	*ʃogwe*

The closer relations of Quechua seem to be confined to Aymara, forming the Quechumaran stock. A lexicostatistic count shows this connection to be at a time depth of about 37 centuries. In these millenia of separation the languages have developed fairly divergent sets of formative affixes, have modified the formation of certain forms (e.g. Quechua *i-ca*, Aymara *ku-chaw* 'if'), have undergone separate semantic shifts (e.g. Q. *phuyu* 'cloud, fog', A. *phuyu* 'feather') and suffered a few positionally restricted sound changes (e.g. Q. *huc'u-*, A. *hut'u* 'small'), but in the main the sound system is the same in both languages and the actual form of cognate elements is often identical (e.g. Q. *kunka*, A. *kunka* 'neck'). Because the words often agree completely, many scholars have attempted to explain all or most of the agreements as due to borrowing. In 1888 Steinthal presented a brief but cogent paper to the Congress of Americanists showing that various important agreements could not be due to borrowing and had to rest on common origin. However, there are still scholars today who consider this an unproved relationship.[24]

11. PATAGONIAN AND FUEGIAN LANGUAGES. In 1892 Brinton published several fragments of vocabulary of Patagonian and Fuegian languages, along with a comparative analysis which carefully distinguished probable borrowings from other similarities.[25] His paper ends with these words: 'The general conclusion to which these comparisons lead is that the Patagonian dialects are probably more closely related than has hitherto been assumed.' The cautious form of the statement is conditioned by the small amounts of material on which it is based, but this should nevertheless have been a valuable lead for more recent scholars who can now operate with much better materials for at least some of the languages. However, Americanists seem to have lost track of Brinton's important suggestion, and continue to list a series of independent linguistic entities, which include Yahgan (Yamana), Alacalufan, Tehuelchean and Puelche. The only progress that has been made is the recognition that Ona and Tehuelche, along with certain other languages or dialects (e.g. Haus, Chon, Kon) constitute a linguistic family. Yet vocabularies now available are sufficient to show that Yamana and

[24] (Heymann) Steinthal, Das Verhältniss, das zwischen dem Ketschua und Aimará besteht, *Compte-rendu du Congrès International des Américanistes, 7e Session*, Berlin 1888 pp. 462–64 (Berlin 1890); Harry Hoijer, Introduction, *Linguistic Structures of Native America, Viking Fund Publications in Anthropology* 6.27 (1946).

[25] Daniel G. Brinton, The Hongote Language and the Patagonian Dialects, *Proceedings of the American Philosophical Society* 30.83–90 (1892).

TABLE 2. *Yamalufan Comparisons*

MEANING	YAMANA	ALAKALUFAN	OTHER LANGUAGES
belly	*celip*	*kacel*	N *ce*, O *car*; N *kasle*, T *le* 'guts'
eye	*tela*	*tel*	O-T *(y)otal*, N *te*, M *ŋe*; O *tel*, N *kates* 'star'
fish	*apur*	*epun*	
ice	*akila*	*akali, alkulke*	N *xwiye*; T *gel*, M *x^{w}iyet* 'snow'
rain	*pilaka*	*aperaʃ*	N *pule*, O *pala* 'cloud'; N *pule*, P *piyim* 'sky'
sea	*pelaka*	*capul, cunpil*	
sun	*lewn*	*lewm, arelok*	O *kren*, T *karenk*
cold	*taruri*	*tureyere*	M *ɨɫe*
all	*miakaʃa*	*tiackacaw*	O *maxes*
big	*ulu*	*akwil*	N *talakw*
blood	*capa*	*kiplayk*	O *sam*, T *caw, saw, san*
dog	*yacala*	*calki*	
drink	*ala*	*acak^{w}ar*	T *kara*; M *ko* 'water'
feather	*optuku*	*terkap*	
five	*kupaspa*	*kupaʃpi*	M *kecu*, O *keʃmer*
fly	*alapa*	*ehelec*	
foot	*koya*	*cekun, likulkul*	T *kal*, N *kala*
good	*ayima*	*layp, lalip*	O *aymere*
grass	*cuka*	*ʃakalip*	M *kacu*; M *kari*, O *korn*, A *arkar* 'green'
head	*lamana*	*lacukal*	M *lonko*
man	*-oala*	*ark*	T *ahal*
mouth	*ya, yin*	*upyele*	M *wun*
neck	*yarek*	*celike, celayike*	
not	*pap*	*paltukul*	
skin	*kawru*	*koʃ, ukulayk*	O *kalpen* 'bark'
sleep	*aka*	*eyakwil*	T *okot*, M *umagn*
small	*yeka*	*yikoat, ikot, cuku*	
snow	*kupanaka*	*akape*	
stone	*awi*	*ketyaw*	
thou	*sa*	*cawl*	T *maʃa*
water	*ʃima*	*cawaʃ*	O *cown*
wind	*uca*	*uruckec*	
woman	*kipa*	*ekinec, ʃiparc*	T *karken*
arm	*kamayn*	*kaynepi*	M *kwi*, N *kwe* 'hand'
knee	*tulapur*	*tulen-tulu*	
hand	*yac*	*yu-kepe; yac-kul claw*	T *ʃike* 'arm'
bite	*yowa*	*yukenayic*	N *yokwah*
moon	*anoka*	*kunhek*	M *kuyen*, Abipon *neoga*
claw	*kalup*	*yac-kul*	T *kacwel*
red	—	*kiru, kiyu*	T *keyunen*, M *kel*

Alacalufan comprise a stock, which belongs together in a grouping, at perhaps the mesophylum level, with Tehuelchean, Mapuche (Araucanian) and Guaycuruan. Puelche, as we have seen, goes with Quechumaran and Tupian, but it is possible that the two microphyla thus outlined may eventually be related at a somewhat greater time depth.

Table 2 shows Yamana-Alacalufan comparisons flanked by apparent cognates in Ona, Tehuelche, Mapuche, Mataco (symbolized by N in the table) and Pilaga.[26] These come from very limited vocabularies, which scarcely provide 100 pairs of words between any two languages. It should be noted that Alacalufan seems to be a family made up of a few different languages, which are not separated in the literature. No attempt has been made here to identify the language of each Alacalufan word because to do this properly would require a great deal of research, preferably in the field. There is considerable phonetic fluctuation in the sources for Yamana, Alacalufan and Tehuelche, and the writing in the Table represents an effort to get at a correct phonemic rendering. For example, since *p* and *f* are often found for the same words in Alcalufan, *p* has been used everywhere.

Besides the comparisons listed in Table 2, we made note of a number of others among Mataco, Mapuche and Ona, as follows:

hand	*kwe*	*kwi*	(*cen*)
not	*ka*	*mika*	*kanyer*
sleep	*ma-*	*umagn*	*misten*
I	*yam*	*ince* (**yamce*)	*ya*
we	*yamel*	*incin*	*yikowa*
what	*aci*	*cem*	(*kif*)
where	*ci*	*cew*	(*kif*)
sharp	*yuy*	*yuŋen*	(*orcater*)
kill	*lon*	*laŋimn*	(*kof*)
man	*hino*	*wenʈu*	*Yahgan win* ('person')
father	*ohca*	*cao*	(*hayn*)
tree	*hala*	(*mamil*)	*harnecon*
fire	*itah*	*kiʈal*	Alakaluf *tetal*
heart	*toll*	(*pinke*)	*tol*
burn	*wun*	(*livlivn*)	*wonkyen*
far	*toxwa*	(*aldinpulen*)	*toon*
hand etc.	——	*mari* 'ten'	*kefmer* 'five', Yahgan *mar-po* 'hand'
see	*wen*	*pen*	(*hayken*)
demonstrative	*tah* 'this'	*tvey* 'that'	*tamna* 'this'
		teye 'he'	*tay* 'he'

[26] Pilaga vocabulary furnished by Jules Henry; R. J. Hunt, Mataco-English and English-Mataco Dictionary, *Etnologiska Studier* 5 (1937); Mapuche (or Moluche) vocabulary from Federico Barbará, *Manual de la Lengua Pampa*, Buenos Aires 1944; collation of Alacalufan vocabulary in John M. Cooper, Critical Bibliography of the Tribes of Tierra del Fuego and Adjacent Territory, *Bureau of American Ethnology Bulletin* 63 (1917); Tehuelchean from José María Beauvoir, *Los Shelknam*, Buenos Aires 1915.

wash	*leh*	(*kican*)	T. *lehe* 'water'
other	P. *lkaya*	*ka*	*kac*
person	(*lam*)	*ce*	*con*
tooth	(*cote*)	*voru*	*ohor*
woman	(*acina*)	*kure*	T. *karken*

If we apply the CVC test, we find about 8 adequate instances for Yamana-Alacalufan, three for Mataco-Ona ('tree, heart, cloud'), and two each for most of the other pairs of languages. The intersection test is needed to demonstrate relationship. The simple CVC proof applied in chain fashion is thus less satisfactory than for much of the Hokogian phylum, but it does not follow that the present grouping is necessarily of greater antiquity than Hokogian. The main difference lies in the fact that Hokogian is conveniently provided with intermediate languages between those more distantly related. This implies either that a lesser number of tongues were lost during the period of differentiation or that a good deal more of Hokogian differentiation took place in the original territory with the dialects in continued contact.

It will be interesting to reexamine the relationship of the Patagonian-Fuegian phylum when it can be done on the basis of a larger number of better recorded vocabularies, and particularly after the great Guaycuruan language complex has been studied in detail and used as a basis for the reconstruction of an older period.

12. Conclusion. The foregoing rapid exploration of methodological and reconstructive possibilities in the New World, the author believes, has succeeded in showing that there are sound methodological possibilities for solving the problems of proof at remote time levels which one encounters in Amerindian comparative linguistics. Further, that much remains to be done in this field and that the lack of data is a principal obstacle in the way of accomplishing it. A significant part of these problems can be effectively attacked by assembling a modest amount of vocabulary and basic structural data on the hundreds of American languages, provided the choice of data is made in such a way as to give the most important vocabulary for comparative purposes and that the material for each language covers similar grounds.

Denver, Colorado

LINGUISTICS AND PREHISTORY

ANTONIO TOVAR

1. If, as is usual, we trace back modern linguistics to the appearance of the works of Bopp, Rask and Grimm,[1] it is undeniable that comparison, the key to the new science, leads us back to prehistoric times. The tendency, however, was of earlier origin than these works, and from J. J. Scaliger to Hervás,[2] at the very beginning of the modern era, the attempt was to determine the first languages of origin. It has always been known that language was one of the most important keys to the origins of humanity. The idea of a primitive Hebraism[3] corresponds theologically to the concept that language conserves the memory of the most remote past.

But linguistics offered the possibility of reconstructing *Ursprachen*, surely and coherently, when prehistory was still engaged in problems of geological chronology and when the perspective of the millennia which we must calculate for the existence of man on the earth had not entered into scientific consciousness.[4] Therefore, today we discover with surprise that only a century ago, when comparative grammar was able to supply a complete picture of Indo-European or Semitic, the scholars who were concerned with the origin of language thought that the "primitive" patriarchal state, the very dawn of humanity, was reflected in these better known branches. Compare E. Renan's text in his *Origine du langage* (1848), where this general opinion is expressed: "Plus on pénétrera dans la connaissance de la haute antiquité des peuples ariens et sémitiques, plus on verra se dessiner dans l'apparente uniformité du monde primitif des figures de sages, d'initiateurs, de prophètes sans nom, auxquels les lois, les moeurs, les institutions de la vie civile et réligieuse, les poésies sacrées se rattacheront comme à leurs inspirateurs. Derrière le langage, on verra de même le *Richi*, le sage primitif, interprète du génie de sa race; on reconnaîtra l'influence de certaines corporations, de certaines familles privilegiées; on trouvera l'école remontant presque à l'origine du monde."[5]

Bearing this in mind, it is easy to understand that linguistic reconstruction,

[1] V. Thomsen, *Hist. de la lingüística* (Span. transl., Barcelona, 1945) 63ff.; H. Pedersen, *Linguistic Science in the Nineteenth Century* (English transl. by Spargo), 39ff.; Tovar, *Lingüística y filología clásica, su situación actual* (Madrid, 1944), 67ff. To begin with, we must recognize with L. Hjelmslev, *Prolegomena to a Theory of the Language* (Suppl. to *IJAL* 19, 1) 2, that our subject still belongs to historical linguistics, i.e., to Philology, except that we do not stress it negatively.

[2] Thomsen, *op. cit.*, 49ff., 58ff.

[3] Thomsen, *op. cit.*, 53ff. We may record here the Spanish Hebrew scholars of the 16th century: Fray Luis de León expresses the belief that the name must "avecinarse a la cosa," which "casi siempre se guarda en hebreo, la primera lengua de todas" (*Nombres de Cristo*, 1.1). The same opinion is sustained by Arias Montano and his translator into Spanish, the monk José de Sigüenza.

[4] Cf. H. Breuil, *Anthropos*, 38–40 (1942–45).667ff.

[5] Page 24f. of the 7th edition, Paris, 1889; see also p. 236.

the determination of primary languages, suffers from an abstraction, with no connection in reality. Due to the lack of an organized prehistoric science, such linguistic reconstruction arose without the possibility of reference to it. In the classical books of comparativism, in Brugmann or Delbrück, common Indo-European, beyond Homer or the Rigveda, is a pure abstraction. Even today E. H. Sturtevant tends to abstraction in his summary of the prehistory of Indo-European (and its relation to Hittite), with no reference to place or time.[6]

The progress of prehistory only gradually influenced linguistics, which, firmly grounded and embedded as it was in its scientifically deduced abstractions, did not find an approach to reality necessary. The work of Boucher de Perthes begins only in 1837, and a short time before the works of the Belgian Ph.-Ch. Schmerling (1833–34) had appeared. The precursors (Mercati about 1593, but published only in 1717; Conyers in 1700, John Frere in 1797) were not generally accredited, or were merely geologists concerned with the age of the earth and the most remote times of man. The first chronological classification is that of Mortillet, about 1880; the study of the more modern stages of transition between the paleolithic and the neolithic was begun by the Portuguese Pereira da Costa (1865) and Carlos Ribeiro (1880) and the Frenchman E. Piette (1887).[7]

But the problem of connecting prehistoric cultures with definite peoples did not arise until the study of the prehistoric ages of metals progressed. K. Wilhelmi in Southwestern Germany, and Fr. A. Wagner (1833), J. F. Danneil (1836), and in England J. M. Kemble (1863),[8] begin to relate the discoveries of Germanic antiquities with peoples belonging to history. Only from this time was it possible to talk of prehistoric linguistics.

Until this moment arrived, the comparative method followed by linguists reconstructed strongly unified common stages, of indefinite duration, situated in abstract space, and was not interested in comparing them with reality. Even when reality was more accessible, as in Romance linguistics, the comparatists, in lieu of the complex Latin of inscriptions or of writers affected by the speech of the vulgar or of faults in manuscripts,[9] sought a comfortable and abstract "Vulgar Latin," as uniform as possible. If abstraction was preferred where reality was near at hand, what could we expect in a situation in which reconstruction led to an unknown world and to an epoch only gradually entering the ken of prehistorians?

2. The appearance of the work of O. Schrader, *Sprachvergleichung und*

[6] *Language*, 28 (1952).177ff. The current skepticism is to be found in works such as F. de Saussure, *Recueil des publications scientifiques*, 396f.; J. Vendryes, *Le langage* (Spanish transl.), 403; N. S. Trubetzkoy, *Acta linguistica* 1.82f., was equally hypercritical about the correspondence between linguistics and ceramics as late as in 1939.

[7] See the precise indications by Breuil-Lantier, *Les hommes de la pierre ancienne* (*Paléolithique et mésolithique*) (Paris, 1951) 7ff.; E. Wahle, Geschichte der prähistorischen Forschung, *Anthropos*, 45.487ff., 46.49ff.

[8] E. Wahle, Zur ethnischen Deutung frühgeschichtlicher Kulturprovinzen, *Sitzungsberichte der Heidelberger Akademie*, Philos.-hist. Kl., 1940/1,2. 52f.

[9] An exception is H. Schuchardt in his thesis *Der Vokalismus des Vulgärlateins*, expanded into three volumes, Leipzig, 1866–68. But such an attempt has remained isolated and without continuators.

Urgeschichte,[10] if it did not pose the whole problem, had at least the design of collating the results which prehistory, on the one hand, and linguistics, on the other, offered concerning the problem of the origins of the Indo-European speaking peoples. The method used by Schrader, set down in his *Reallexikon der indogermanischen Altertumskunde*,[11] was followed by all the investigators, mainly Germans,[12] like S. Feist,[13] H. Hirt[14] and lastly F. Specht.[15] The *Festschrift Hirt*, published under the title of *Germanen und Indogermanen, Volkstum, Sprache, Heimat, Kultur*, on the eve of the second World War, undoubtedly offered a broad perspective of the relations between linguistics and prehistory, but its limitation consisted in the linguistics being fundamentally a comparison of the Indo-European languages, and the prehistoric problem being restricted to the Indo-Europeans and their immediate neighbors. The panorama is identical, for example, in L. Bloomfield,[16] who deals only with prehistory in connection with Indo-European linguistics.

The crisis of contemporary linguistics, to which it is convenient to refer in this examination of the problem, is due fundamentally to the fact that we have to recognize with L. Weisgerber[17] that (and we must add: above all in Germany) "bis in die jüngsten Veröffentlichungen hinein wird auch in den wissenschaftlichen Darstellungen *Sprachvergleichung* . . . als durchaus gleichwertig mit *indogermanischer Sprachwissenschaft* gebraucht." If, as we have seen, linguistics preceded prehistory in penetrating the question of origins, prehistory for its part first attained to a universal conception;[18] linguistics was probably afterwards left behind in this respect. A quarter of a century ago, the *Reallexikon der*

[10] First edition 1883, third and last 1906–07.

[11] First edition 1901, 2nd revised by A. Nehring 1929.

[12] We must record here a precursor in the French language, A. Pictet, *Les origines indo-européennes, ou les Aryas primitifs, essai de paléontologie linguistique*, 1859–63, 2nd edition 1877. This Swiss writer develops the aspects of material culture and is a direct predecessor of V. Hehn's famous book *Kulturpflanzen und Haustiere in ihrem Uebergang von Asien nach Griechenland und Italien sowie in das übrige Europa* (first ed. 1870, eighth revised by O. Schrader 1912). The large work by W. Schmidt, *Rassen und Völker in Vorgeschichte und Geschichte des Abendlandes*, 1946–49, is a modern transposition, on a cultural-anthropological basis, of these problems, especially in their relationships to the origins of the Indo-European peoples.

[13] *Kultur, Ausbreitung und Herkunft der Indogermanen*, 1913.

[14] For this author see the collection of papers, edited by his pupil H. Arntz, *Indogermanica, Forschungen über Sprache und Geschichte Alteuropas*, 1940. Cf. also Arntz's review of the ideas of Hirt in *Germanen und Indogermanen*, 2.25ff.

[15] *KZ* 64 (1937).1–23, 66.1–74 and 196f., and lately *Der Ursprung der indogermanischen Deklination*, 1944; the criticism of P. Arumaa (*Apophoreta Tartuensia*, Stockholm, 1949, 73ff.) on the last book is very interesting but too harsh.

[16] *Language* (London, 1950) 319. A recent resumé by A. Scherer is to be found in *Preliminary Reports* to the Seventh International Congress of Linguists, 161ff. See also V. Pisani, *Allgemeine und vergleichende Sprachwissenschaft—Indogermanistik, Wissenschaftliche Forschungsberichte*, 2 (Berne, 1953).97ff.

[17] *Lexis*, 2.4.*

[18] For instance, O. Menghin, *Weltgeschichte der Steinzeit*, 1931, and other works directly inspired by the Vienna school; cf. Pia Laviosa Zambotti, *Origini e diffusione della Civiltà*, 1947.

Vorgeschichte of M. Ebert could present linguistic results clearly and surely for historical peoples, but it lacked all connection with anything related to prehistoric cultures. This was still the general opinion at that time.

3. The aim of prehistory is to arrive at an explanation of what happened in remote times, at least along general lines. As it advances towards such an end, prehistory gradually becomes history. In this way linguistic prehistory loses ground each time that new texts are transformed into genuine written documents by decipherment. The "pre-Hellenic civilizations" become objects of history when they are explained by deciphered texts, like those written in Linear B after M. Ventris' reading. The prehistory of Greek dialects becomes history to the extent that it is explained and documented by these texts.

This happened with great frequency in studies on the Near East, where the achievments gained by European science in a century and a half constitute one of the most brillant triumphs of the human mind. Thanks to these investigations, knowledge which we may call strictly historical has made astonishing advances. The Bible, the Vedas and the historiographical tradition of the Ancients are modern documents by the side of Sumerian and Egyptian texts. The important thing for us, however, is not the transformation of prehistory into history through the reading of texts, but the degree to which linguistics may be used to throw light on those parts of prehistory for which texts are scarce, obscure or completely lacking. In this case we have to look for linguistic elements elsewhere than in texts and documentation.

With the example set by the decipherers of Egyptian or cuneiform inscriptions, linguists labor at times with the most successful results on scant and difficult remains of ancient cultures which have disappeared. All that we know about Asia Minor, for instance, reconstructing a score of languages on the basis of a few inscriptions, and the successes of linguistics on the epigraphy of those peoples, exceeds what could have been expected with such fragmentary material.[19] The same thing has occurred with Italian origins, in spite of the difficulties with which the Etruscologists have still to contend.[20]

Similarly the results of linguistic investigation on the Hispanic peninsula, following the solution of the first epigraphic problems, clear up to a large degree prehistoric periods which are much more scarcely documented in historiographical traditions. My *Estudios sobre las primitivas lenguas hispánicas*[21] represent the

[19] Laroche gives an authoritative survey in *Conférences de l'Institut de Linguistique de l'Université de Paris*, 9 (1949) 65ff.

[20] R. S. Conway–J. Whatmough–S.E. Johnson, *The Prae-Italic Dialects of Italy*, 3 vols., 1933, is the standard collection of materials, and G. Devoto's work *Storia della lingua di Roma* is one of the most important and conclusive; for the Etruscan question, we can refer to M. Pallottino's books and papers, specially *Etruscologia* 1947, *Archiv Orientální* 18 (1950) 159ff., and recently *Testimonia linguae Etruscae*, 1954.

[21] Published by the University of Buenos Aires in 1949 as a collection of papers which appeared previously in different journals. In it is established the linguistic map of the peninsula; Celtiberian is first tentatively deciphered, etc. Reviewed by M. Lejeune, *BSL*, 51.78ff., R. Lafon, *Bulletin Hispanique* 54.165ff., L. Michelena, *Emerita* 20.545ff., V. Pisani, *Paideia* 6.152ff., J. Whatmough, *Language* 27.571ff.

end of a stage, initiated by Gómez-Moreno in 1922.[22] Results which may be considered definitive are: the attribution of the northwestern half of the peninsula to the Indo-European world and the survival of pre-Indo-European languages and peoples in the whole of the East and South, from the Basque country to Almería and Lisbon, although with very important penetrations of Indo-European elements.

It is the scarcity and limitation of linguistic documentation which, even after the material has been read and deciphered to some extent, determines whether a prehistoric epoch shall become completely historic or not. It is in these areas that linguistics still has much to accomplish. Thus the linguistics of Gaul remains prehistoric, even after the material of its inscriptions has been studied by J. Whatmough.[23]

We are face to face with linguistic prehistory in the most ancient Germanic runes[24] and in the Oghamic monuments,[25] and also in the barbarian onomastics of Roman inscriptions. The interpretation of such linguistic remains throws light on neighboring obscure zones, situated beside those clarified by historic documents. Archaeological remains, genealogical traditions and the data of religious history are best explained by words themselves.

Linguistics has gained a comparable triumph in its work upon completely historic eras whose historiography affords us only few data in dry chronicles. The whole of social and linguistic life, the changes in population, the reconstruction of their economic history[26] and cultural relations, may be deduced from linguistic history. The use made by Menéndez Pidal in his work on the *Orígenes del español*[27] of linguistic methods has clarified not only the linguistic history of Spain, but political and social history itself.

The subtitle of this work, "Estado lingüístico de la Península Ibérica hasta el siglo XI," is a strong indication of the author's intention in studying an epoch with scarce documentation. With reference to one particular chapter, R. Lapesa's booklet *Asturiano y Provenzal en el Fuero de Avilés*[28] very significantly discovers in a Medieval Spanish text forms of predominantly Southern French dialects which unexpectedly illustrate the importance of the French element in the

[22] Within this period are the works of Caro Baroja, Vallejo, J. Urquijo, P. and A. Beltrán, G. Bähr and A. Schulten. Before Gómez-Moreno scholars worked on Iberian inscriptions without knowing the writing: from F. Orsini and Antonius Augustinus (16th century) until Hübner and Schuchardt. Linguists are at times not yet aware of this idea, and it surprises us for instance that J. Whatmough (*Language* 27.574) relies on Schuchardt, who worked on Hübner's readings which are not admissible today.

[23] *Dialects of Ancient Gaul*, 5 microfilm rolls ed. by the University of Michigan, Ann Arbor 1949–51; formerly the standard work was G. Dottin, *La langue gauloise*, Paris 1920.

[24] Cf. D. Diringer, *The Alphabet*, 507ff.; J.G. Février, *Histoire de l'écriture* 501ff.; F. Altheim and E. Trautmann, *Vom Ursprung der Runen* 1939.

[25] R.A.S. Macalister, *Corpus inscriptionum insularum Celticarum*, 1, 1945.

[26] A curious example studied by P. Lebel is the felling of trees in a forest, as it is reflected in toponymy, alluded by J. de Castro-Nunes, *Revista de Guimarães* 63 (1953) 474ff.

[27] First edition, Madrid 1926; 2nd 1929; 3d 1950.

[28] *Acta Salmanticensia* Filosofía y Letras II 4, 1948. See especially the linguistic map of this work.

repopulation and reorganization of the Christian Kingdoms in the Reconquest, and the interpenetration of this element in the Spanish municipalities.

4. Linguistics sheds a great deal of light upon periods either unilluminated by history, or with insufficient historical documentation. When there is no documentation, linguistic data are no more than indications, sometimes of difficult interpretation and always of uncertain absolute chronology.

For instance, the discussion between T. F. O'Rahilly[29] and J. Pokorny[30] about the disposition of the Celtic layers in Ireland is an interesting example. For the former, Britons or *p*-Celts were the oldest invaders, whereas, for the latter, the *q*-Celts. Linguistic interpretation alone is hardly capable of fully solving such problems. For example, a third party in this discussion, J. Whatmough,[31] asserts that Pre-Celtic Indo-Europeans, as a layer preceding the two said Celtic invasions, did not exist in the West. The solution of such problems lies in identifying the archeological materials with the linguistic groups, i.e., with peoples. The task is not so easy and is open to the greatest risks, but nevertheless it is the only method of historical penetration into prehistory.

If we return to the prehistory of the Iberian Peninsula, it is thanks to linguistics that we can speak today of the presence of Indo-Europeans, and archaeology, together with certain general considerations of a cultural nature,[32] facilitates, by the analysis of forms of material culture and decorative elements, the task of determining the extension of the invasions and their predominance, even where they were culturally partially absorbed.

The interpretation of cultural forms is very revealing, provided that it is not used arbitrarily. Thus, the analysis of decorative forms in Iberian ceramics shows the penetration of Indo-Europeans even into the region of Eastern Spain, to be compared with the inscription *saguntico*[33] on a vase from the same region, and the presence of the name *Mandonius* amongst the Iberians of the river Ebro or the Basque *mando* 'mule,' borrowed from the Indo-European.[34]

In the same way, in the problem of the "Indo-Europeanization" of Greece,[35]

[29] *Early Irish History and Mythology*, Dublin 1946, to whom for instance A. M. Heiermeier, (*Journal of the R. Society of Antiquaries of Ireland* 82 [1952]. 37ff.) has adhered.

[30] An answer to the opposite theory, with references to former publications, will be found in an article under the title Probleme der keltischen Urgeschichte, *Congrès International des Sciences Préhistoriques et Protohistoriques*, Actes de la III[e] session Zürich, 1950 (pub. 1953). 281–84. See also the same scholar's *Keltologie, Wissenschaftliche Forschungsberichte*, 2 (Berne, 1953). 103ff.

[31] J. Whatmough, *Harvard Studies in Classical Philology*, 55.18,38f.; *Language*, 27.574.

[32] J. Maluquer de Motes, *Zephyrus* 5 (1954) 1ff.; idem, Frühe indoeuropäische Häuser im Ebrotal, *Germania*, 31 (1953) 155ff.; idem, *El yacimiento hallstattico de Cortes de Navarra*, 1 (Pamplona 1954), especially 181ff. The latest of these works, 158ff., points out the Central European origin of some decorative patterns found in this site and also their diffusion in the Iberian art of Eastern Spain.

[33] Tovar, *Estudios dedicados a Menéndez Pidal*, 2 (Madrid 1951) 316.

[34] See my *Estudios sobre las primitivas lenguas hispánicas*, 154ff. Another exemple, very problematic, but highly interesting, is the explanation given by L. Michelena, *Emerita*, 20 (1952) 153ff., of the Iberian *kirsto* as an IE sigmatic aorist, meaning 'made'.

[35] M. Nilsson, *The Minoan-Mycenian Religion and its Survival in Greek Religion*, (Lund

linguistics is our principal guide to the eras previous to the Doric invasion and geometrical culture. In so far as it is possible to make a genealogical classification,[36] the study of Greek dialects helps us most to put the invasions in their true perspective. It is true that the "historification" of this period, which until recently was prehistoric, owes its initiation to Mr. Ventris' discovery of the key to the Linear B writing. In view of the first discoveries, in which we read forms like *a-ni-ja-pi* (*ἀνίᾱφι*), *qe-to-ro-po-pi* (*κʷετροπό[δ]φι*),[37] it seems reasonable that the people who spoke such a language were Achaeans. This does not mean that the Ionians had not arrived before them in Greece.[38] In 1944 I supposed that the first Hellenic invaders, the Ionians, could be identified with the makers of the ceramics which Furtwängler called Minyan, and modern archaeologists call Mattmalerei. Only the identification of prehistoric material cultures with definite peoples can historically classify vast zones of prehistory. This identification is confirmed when epigraphy provides documents susceptible to a more or less complete linguistic interpretation.

5. Besides epigraphical documents, linguistics can help to interpret prehistoric data, but only by special methods, not accepted universally. If it were possible to ascertain, with general approval, the linguistic nature of Nordic culture, Schnurkeramiker and Bandkeramiker,[39] without doubt the theme of Indo-European origins would gain in precision and clarity, in spite of its rather theoretical and abstract character. Utopia and Uchronia have been the dangers of the theme, notwithstanding the cultural data drawn from the language itself. To determine with certainty the identity of an archaeological layer the key lies

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1950), 11ff., has written particularly interesting considerations on the question of the coming of the Greeks. See too, among the newest publications, P. Kretschmer, *Miscelanea Academica Berolinensia* (1950), 173ff.; Otto Haas, *Lingua Posnaniensis* 3 (1951) 63ff.; W. Brandenstein, *Griechische Sprachwissenschaft*, Berlin 1954, 5ff. V. Pisani (*Paideia*, 9.13) accepts and continues this explanation.

[36] I have relied on P. Kretschmer, *Glotta*, 1.9–59 in the first part of a study published in 1944 (Ensayo sobre la estratigrafía de los dialectos griegos, I, Primitiva extensión geográfica del jonio, *Emerita*, 12.245–335). We must consider that from the combination of linguistic data with historical indications in the mythology and local traditions, it is deduced that Ionian represents the first Hellenic wave, in great part rejected and vanquished by the second, Aeolian layer. F. R. Adrados, La dialectología griega como fuente para el estudio de las migraciones indoeuropeas en Grecia, *Acta Salmanticensia* Filosofía y Letras, V 3 (1952), has again posed some problems of method.

[37] Ventris-Chadwick, *JHS*, 73 (1953) 100.

[38] *Emerita*, 12.266

[39] To mention some publications, see W. Schultz (Jena), *KZ*, 62.184ff.; H. Seger, *Germanen und Indogermanen*, 1.5f.; Specht, *KZ*, 62,102 (who distinguishes between "older Indo-Europeans," having no deverbative verbs in *ē*, and more recent Indo-Europeans, i.e. Schnurkeramiker, cf. *KZ*, 66.3 and 72). A recent defense of the origin of the Indo-Europeans on Russian soil can be found in the very clear study of A. Scherer, *Archiv für Kulturgeschichte*, 33 (1950).3ff. P. Bosch-Gimpera has made a new revision of the question and is a strong champion of the possibility of identifying material cultures with prehistoric peoples (i.e. languages). For him the Urnenfelder people are Celts, but he does not believe that the Lusatian culture is Illyrian. The Danube culture is, according to him, predominantly Indo-European (*Etudes Celtiques*, 5.352ff., 6.71ff.). Cf. also J. Knobloch, *Lexis*, 3.290.

without doubt in the continuity of culture and race in a particular place. The investigators who claim to trace back to Neolithic times the existence of Indo-Europeans in Germany, insist on the continuity Megalithic–Schnurkeramiker–Germanic. If the continuity is lacking, such theories are left in the air. The question becomes the more serious, given the possibility that where there is no apparent rupture in material culture, there may have existed a political, and even racial, rupture. A recent example is the discovery of Greek texts in Late Minoan. It is now certain that "a Greek aristocracy, despite stylistic indications of Minoan continuity, was established at Knossos in L. M. II."[40]

For prehistoric epochs (or historical, with a scarcity of data which in any case are always too external) the study of names, whether of places, persons, or gods etc., has a particular value.

Personal names are of cultural significance, while those of places may easily stem from genuine patrimony and admit a prehistoric interpretation. Names of persons and places have always served to explain origins. The Ancients made use of them, and the theory of primitive Hebraism was also based in them. Basque scholars sought in place-names the proof that Basque was the primitive language, in the same way as Celtomaniacs did in their turn. W. von Humboldt improved the onomastic method in his famous *Prüfung der Untersuchungen über die Urbewohner Hispaniens vermittelst der Vaskischen Sprache* (1821). Undoubtedly P. Kretschmer's *Einleitung in die Geschichte der griechischen Sprache* (1896) is the first work to base on toponymy and onomastics the construction of a vast picture in which multiple elements and long historical epochs intermingle. Thus, the whole Aegean area was now considered in a new light. The foundations were now laid for later studies.

Through onomastics in general we may discover one people (i.e. a language) beneath another which obscures it historically. The presence of a *Pohoidan* in Laconia teaches us that, with a Laconian change of *s* to *h*, a cult survives there which is not Dorian, but older, belonging to dialects (Ionian, Achaeo-Aeolian) in which there occurs assibilation, as opposed to Dorian *Poteidan*.[41]

By means of personal onomastics M. Gómez-Moreno was first able to indicate the general lines of the ethnology of the Hispanic peninsula, with an approximate notion of the distribution of peoples and languages, confirmed by later studies.[42]

Onomastic sciences have made possible the analysis of the layers of Indo-European population in the center of Europe with the resultant pointer to a Veneto-Illytrian component, which seems to correspond to the culture of the Urnenfelder.[43]

[40] Ventris-Chadwick: *JHS*, 63.84.

[41] On the subject of toponomastics and their value, V. Pisani, *Paideia*, 1.277ff. For the special point referred to in the text see P. Kretschmer in Gercke-Norden, *Einleitung in die Altertumswissenschaft*, 1_3.80 (=*Introducción a la lingüística griega y latina*, Madrid 1945, 160); Ventris-Chadwick, *JHS*, 63.95 and 103.

[42] *Homenaje a Menéndez Pidal*, 3 (Madrid 1925).475ff.=*Miscelánea*s (Madrid 1949).233ff.

[43] J. Pokorny, *Zeitschrift für celtische Philologie* 20.315ff., 489ff., 21.55ff.; R. Pittioni, *ibid.*, 21.167ff. The question remains very dubious and Bosch-Gimpera has marked a posi-

Recently H. Krahe has analyzed European hydronymics, from Italy to Norway and from the Atlantic to beyond the Polish plains, arriving at the conclusion that[44] there existed a homogeneous and very ancient layer of Indo-European names belonging to a primitive population, at that time still unified and only afterwards becoming differentiated in Italic dialects, Germanic, Illyrian, Venetic, and Baltic.

These examples of the success of toponymy suffice to prove how the linguistic analysis of onomastic materials is one of the most effective collaborators of prehistory. Further, it is even capable of extracting history from prehistory. In toponymic fossils and in the preservation of names a long time after the disappearance of their language of origin (for instance, Germanic names in Romanic nations), we have indications to clarify epochs for which there is no historical documentation.

6. At this stage it is well to consider a vexed question, which it is now the fashion to minimize: substratum. We consider of course that the conception of racial substratum, consisting in a certain atavistic predisposition or hereditary articulatory habits is now eliminated.[45] But language is completely independent of the various physical conditions deriving from racial characteristics. Even the attempts to establish an "archaic" type of language dispense with traits which depend on the marked differences characterizing for example an Australian.

We must conceive of substratum in a purely linguistic sense, based on a historical and social circumstance: bilingualism. We owe to Meillet the indication of the correct path, discovered by him, however, rather a propos of the superstratum.[46] It is only through an epoch of bilingualism that we can explain the survival in one language of phonetic, morphological, syntactical and above all lexical traits of another, superseded or absorbed.

We have proof that the persistence of indigenous languages[47] in Latin is what colors and diversifies the supposedly unified Latin, which, for comfortable reasons

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tion against this "Illyrism." Whether these peoples must be called Illyrian or not, is difficult to decide, but I think the presence of a "Pre-Celtic" and, besides, Indo-European people in Central and Western Europe is beyond question.

[44] See the articles of this author in *Beiträge zur Namenforschung*, 1.247ff., 2.113–31, 217–37, 3.1–18, 153–70, 225–43, and specially his lecture *Sprachverwandtschaft im alten Europa*, Heidelberg, 1951. The criticism of W. Winter, *Language*, 28.271, seems to me excessive.

[45] There is a sound criticism of these ideas in Menéndez Pidal, *RFE*, 34 (1950).1ff. In *Acta Linguistica*, 7 (1952).1ff. we find a study by N. Å. Nielsen, whose general contention is excellent, but the presentation of facts is completely oldfashioned.

[46] *Linguistique historique et linguistique générale*, 2.90ff., 98ff.

[47] The inscriptions of La Graufesenque (F. Hermet, *La Graufesenque* (*Condatomago*), 2 vols., Paris, 1934, J. Whatmough, *DAG*, 2.274ff.) prove that Gaulish was still spoken under the Empire, and for Spain we have, for paleographical reasons, allotted to the 2nd century A. D. the inscriptions of Peñalba de Villastar (Gómez-Moreno, *Miscelánea*s, 326ff.; M. Lejeune, Celtiberica, under press in *Acta Salmanticensia*). I have insisted very specially on this question, pointing out the drawbacks for scholars which result from abolishing the limits between Latin and the barbarian languages, and also the introduction of such pre-Romance phenomena at a very early time (*REL*, 29.105). On Gaulish under the Empire, cf. also J. U. Hubschmied, *Vox Romanica*, 3.48ff.

of methodology, was postulated by the Romanists of the old school. Vulgar Latin must have offered certain dialectal differences, and it is only due to the fact that such differences do not tally with the later Romance dialects that the attempts to establish a true Latin dialectology have met with utter failure.[48] But I believe for example that it is proven[49] that the voicing of Western Romance is nothing but Celtic aspiration: that is to say, it appeared in the Latin spoken predominantly by Celts.

Similarly, inflexion in Romance languages coincides approximately with Celtic infection and with the German Umlaut.[50]

To explain such circumstances on the basis of a substratum, understood as ancestral articulatory habits, would be unacceptable, but the fact of the Celto-Latin or Germanic-Celtic bilingualism of the populations of Western Europe, first in extensive territories and later in more remote corners of the Continent, which were later to develop into focuses of active irradiation of a language, suffices as an explanation.

As a solution of the problem, we must employ the concept of slow linguistic evolution, called a latent state (*estado latente*) by Menéndez Pidal,[51] in which a phenomenon does not appear for centuries, but reappears after such an eclipse. The study of popular poetry, or rather, according to Menéndez Pidal's terminology, traditional poetry, seems to show that what seems dead is conserved deep in the soul of the people, that is to say, less romantically, in a hidden geographical area. Popular poetry unrevealed to the investigator or a linguistic phenomenon unapparent in the speech or writing of a given moment, remains latently, only awaiting detection by more acute observer, and ready to emerge when the bonds of official language and the consciousness of tradition become relaxed. This latent state in linguistics justifies the at times very slow tempo in which languages evolve. Sometimes the change is surprisingly rapid, as a resŭlt of great historical catastrophes, whilst at others, the change begins at an unknown time and place, then spreads in a more or less perceptible and documented manner, and finally, only after many centuries, becomes general and systematic.

In this order of ideas, the voicing which we have seen in Western Latin at the very beginnings of the Principate[52] as a consequence of Celto-Latin bilingualism, reappears in the Latin of Leonese notaries of the 10th and 11th centuries, and finally becomes a fixed characteristic of the Peninsular dialects and, in general, of Western Romance, with the unique exception of the areas which conserve intervocalic voiceless stops in Aragonese and Gascon, bordering on the Basque linguistic region, which similarly conserves these intervocalics.

[48] The studies on this theme by Sittl and others here criticized in my article in *REL* 29.106 (quoted above). I find the study of the theme by J. Whatmough, *DAG*, 1.1 and 56ff. very accurate and well expressed.

[49] Besides *REL*, 29.102–20, see A. Martinet, Celtic Lenition and Western Romance Consonants, *Language* 28.192–217.

[50] An article of mine on the question will appear in Actas del VII Congreso Internacional de Lingüística Románica, Barcelona.

[51] *Orígenes del Español*, 533f.; *Romancero Hispánico*, 2 (Madrid, 1953), 361ff.

[52] *REL*, 29.114, 116; Menéndez Pidal, *Orígenes*, 240ff.

In the same way, phenomena observable in Oscan (for example *nn* < *nd*) are reproduced in Italian dialects of the South, and there is an evident parallel between the Gallo-Italic type of the dialects of the North and the existence of Cisalpine Gaul.[53]

Extinct languages live on through their influence as substratum. The passage of the conserved characteristics into the new language is realized during a bilingual stage. The transfer of cultural and political supremacy from one region to another plays a decisive part in assuring the permanence of these characteristics. What was a conservative and rural area becomes the center of irradiation in a new era.

We may also talk of substratum in a more indirect sense, when the traits of an extinct language, partially preserved in a succeeding language, pass from this one to a third, in turn its substitute. For example, perhaps the Western voicing and inflexion ought to be attributed not to Celtic but to the languages displaced by that language. Similarly we may note the lack of *f* common to Iberian, Basque and, in Spanish, to the evolution of *f-* to *h-*.[54]

7. In so far as the problems of prehistory have become clarified in contact with ethnology and cultural anthropology, the study of such sciences has assisted linguistics in its approach to prehistoric problems. North American science first opened the way. Linguists of the European schools, generally restricted to a well-defined group of languages, were able to elaborate methods of pure linguistics,[55] but in America, where the study of languages passed into the hands of ethnologists, the aim in research was, principally, cultural significance or the problem of the interrelationship of languages. The fact that in America languages belong to numerous families, and that the methods of comparison which, in Europe, had led to the reconstruction of the great Indo-European family, had no possibility of success in indigenous America, necessarily produced a sudden and marked divergence between linguists of the American school and the Europeans. The solidity and perfection of positivist methods blinded the vision of European scholars, who rejected any disturbing novelties.

The tendency of traditional linguistics to abstraction, as a principle to separate the concept of race from that of language,[56] often situating the "cradle of the Indo-Europeans" in any indefinite place, and renouncing a strict chronology in the dispersal of dialects, was shown in pure comparison, which despised elements not susceptible to comparison.

[53] C. Pulgram, *Language*, 25.241ff.

[54] Menéndez Pidal, *Orígenes*, 198ff.

[55] It is curious that amongst the masters of linguistics of the foregoing century only A. Bezzenberger cultivated ethnology and prehistory as well as linguistics. But, if we glance at the entire collection of the *Beiträge zur Kunde der indogermanischen Sprachen* directed by him for 30 years, we find that the obituary of K. Müllenhoff by G. Kossina (9.1885.135ff.) is the only writing of interest on this subject, and yet Müllenhoff was predominantly a philologist!

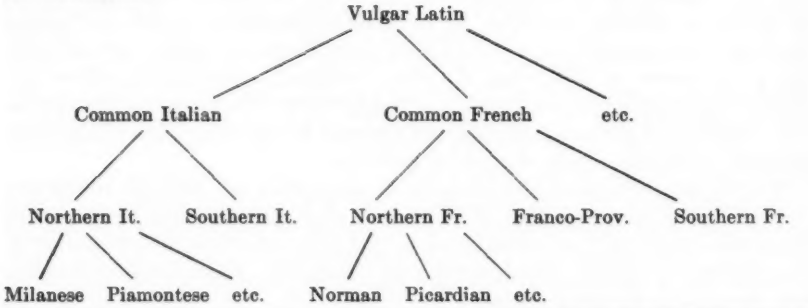
[56] A vigorous criticism against this idea in H. Hoijer, *Language*, 24.335ff.; cf. also F. Boas, *Cuestiones fundamentales de antropología cultural* (Spanish transl., Buenos Aires, 1943) 146ff.

The conception of the world with Europe as its center was reflected in the historic-comparative study within the limits of which such a method was applicable (Indo-European, Semitic, Finno-Ugrian), neglecting the rest. The present linguistic crisis is also largely a modification of our fields of interest.

In so doing, linguistics is incorporated among the sciences concerned with Man,[57] and bases its studies on the prime fact that language is "human" and represents a "cultural" reality. Thus linguistic facts are situated within a social reality,[58] which, if we adapt ourselves to the cultural horizons of peoples at different stages, belongs largely within cultural anthropology.

8. An immediate result of adapting ourselves to the variety of cultural horizons is to gain a new conception of interlingual relations. For those accustomed to the comparison between the most modern languages of the Old Continent, the metaphor of the genealogical tree lives on continually for the sake of explanation and it is difficult to eradicate and always persists in the consideration of some subgroups of Indo-European languages as a bifurcation of one of the former branches. Thus we still speak of an Aryan or Indo-Aryan group, a Balto-Slavic group,[59] an Italic group.[60] Yet a logical consideration of the facts would lead us to criticize these survivals.

We easily understand the scant genealogical significance of such subgroupings if we compare them with the different dialects of an identical linguistic stem in the light of historical reality. The folly of conceiving of a genealogical tree for dialects of unknown prehistory is obvious if we imagine a similar tree in a well known field, such as Romance dialects. The following scheme, for example, would be ridiculous:



[57] H. Hoijer in A. L. Kroeber, ed., *Anthropology Today* (Chicago, 1953).554ff.; R. Jakobson's rapport in Results of the Conference of Anthropologists and Linguists, Suppl. to *IJAL*, 19,2.27f.; chapters XVI and XVII in S. Tax–L. C. Fiselay–I. Rouse–C. F. Voegelin, *An Appraisal of Anthropology Today*, Chicago 1953.

[58] Ch. Bally, *Le langage et la vie* (3rd ed., 1952), 20ff., 115ff.; H. Hoijer, *Language*, 24.335; C. Tagliavini, *La lingua e la società*, Annuario dell'Università, Padova, 1949.

[59] The Balto-Slavic unity was already criticized by A. Senn in L. I. Strakhovsky, *A Handbook of Slavic studies* (Cambridge, Mass., 1949), 54.

[60] Most particularly in the *Storia della lingua di Roma* and other works of G. Devoto. Right now there is a burning controversy on this subject: V. Pisani, *Allgemeine Sprachwissenschaft* . . . , 75; idem, *Rhein. Mus.*, 97.75ff.; and against him J. Lohmann, *Lexis*, 3.206ff.

Such a diagram would be useful, at best, for beginners and as an aid to memory, but it would reflect complicated reality as little as the Indo-European genealogical tree itself, with its Urgriechisch, Urbaltoslavisch, etc.

Therefore, geographical considerations are less abstract than genealogical ones. And thus a linguistic kinship through geographical proximity stands revealed in its full importance. In Europe, E. Lewy[61] has stressed most energetically the importance of this type of kinship. Although often the selection of the criteria to define it is not easy, proof of its existence is afforded every time that we discover linguistic realities which transcend genealogical diversities in particular territories: so we speak of Balkanic traits common to languages which are genealogically at variance.[62] For the same reason Russian and Mordvinian have almost exactly the same phonemes, although they are genealogically unrelated.[63]

The concept of "area" belongs to the same way of thinking, and derives from the methods of cultural anthropology of F. Boas.[64] In this manner we may explain the establishment of certain "families" of languages not susceptible to reduction to a common genealogical type, as T. A. Sebeok[65] has done with the Uralo-Altaic family.

The concept of Sprachbund or linguistic association[66] has been created in the style of ethnologists. Apart from its immediate interest and its relation to cultural anthropology, it facilitates our understanding of possible linguistic interrelationship in prehistoric times.

In fact, we tend spontaneously to consider the relations between languages as similar to those which exist in the mind of the linguist himself. For the linguist, as for the cultured polyglot, who passes from one system to another like a musician changing key, languages are closed and autonomous systems. But the case of the polyglot is different at another culture stage, in a more primitive society. The Caucasian peasant who speaks various languages[67] or the American Indian who acts as interpreter does not isolate the various linguistic systems he uses. The case of the bilingual child is similar. As a result languages in such a situation are interrelated in a completely different way from written and more developed languages.

The bilingual person gives us a series of interesting facets which, viewed from the higher position of a language of culture, seem anomalous to us, but yet are normal, not only in more primitive cultural stages, but also in the life of

[61] Der Bau der europäischen Sprachen, *Proceedings of the Royal Irish Academy* (Dublin, 1942), 48 C 2 and *Anthropos*, 45.49ff. For a new conception of kinship in linguistics I find important ideas in V. Pisani *Archiv Orientální* 17 (1949).251ff.

[62] Besides the well known work of K. Sandfeld, *Linguistique balkanique*, Paris, 1930, on the phonetic side, B. Havránek, *Arch. néerl. de phonét. experim.* 8/9.119–25. finds the same vowels in all languages of the Balkan Peninsula.

[63] N. Trubetzkoy, *Charisteria. . .Mathesio* 21–24.

[64] C. F. Voegelin, *Word*, 1.54ff.

[65] *Lingua*, 2.137.

[66] The inventor of this expression was Trubetzkoy; see Schwyzer, *KZ*, 68 (1943).98, who criticizes the term and prefers "sprachliche Affinität."

[67] H. Vogt, *NTS*, 12 (1942).250. For a classification of bilingualism in types see R. Jakobson, *Results*, 24f.

ordinary people. "Mixture of languages," "borrowing," substratum, "creole languages," hybrid language, are nothing more than "the carriers of interlingual contagion."[68] Social pressure imposes the language on the newcomer to a linguistic community, and thus he becomes a bilingual person. But this does not happen without the two languages in collision being affected to a lesser or greater degree. When an American Portuguese[69] changes *closet* into *clauset* through the influence of *claustro clausura*, or forms *alverozes* from *overalls* (analyzing the *o-* as his native article and substituting it by the Arabic, which is so common in Portuguese), he brings to American English a Latin and Arabic substratum.

The primitive condition is polyglottism; monolingualism is a cultural development.[70] "Purism," which appeared in an acute form in European languages as they became literary or national, is only the desire to stress that stages of inferior culture, proper to unwritten languages, have passed. There is a will to point out that the stages in which languages coexist and intimately intermingle their lexical and grammatical elements have been superseded.

Such is the type of relation between the languages which we have proposed should be called "protohistoric" or "prehistoric."[71] Possibly we may have to determine this distinction to the extent which the study of peoples of a more or less primitive culture allow such determination. Provisionally it is in protohistory that we may best compare these types of relation.

Since languages are not living beings, kinship is reduced to a pure cultural relation.[72] Expressions like "sister languages" are simply metaphors. The continuity between Latin and French or Latin and Italian, stressed by Meillet,[73] opens our eyes to the significance of such metaphors of kinship. The Ursprachen are broken up into dialects, which in turn are recast into national or common languages, but the different grammatical and lexical elements remain fluid. The tendency to a system, at times perfect and coherent, whilst at others less so, is the force which unifies them.

The cultural situation may be such that the related languages, that is to say, the languages more or less unified or bound by the same tradition, form continued geographical areas. In this case, such areas show a gradual differentiation in the dialects, and heterogeneous languages are eliminated. This is an external sign that we are dealing with languages which have entered into a fully historic

[68] E. Haugen, *Lingua*, 2.271ff. See too A. Grégoire, *ibid.*, 355–98 on children's linguistics. On the question of mixed language, cf. A. Rosetti, *Acta linguistica*, 5 (1945/9).73ff. An analysis of a Creole language by R. A. Hall Jr., *Ricerche linguistiche* 1 (1950). 200f.

[69] Leo Pap, cit. by Haugen 283

[70] Tovar, *Actes du 1er Congrès de la Fédération Internationale des Associations d'Etudes Classiques* (Paris, 1951) 57.

[71] *Ibidem:* "Quand les langues d'un espace déterminé ne sont pas des langues de culture, cette coexistence est précisément ce que nous appellons promiscuité. . . . Dans cet horizon de la préhistoire les langues ne sont pas aussi fermées que dans les cultures supérieures. Donc des emprunts de toute espèce se réalisent entre les différentes langues, qui territorialement ne forment pas des districts fermés."

[72] Terracini, *Guida allo studio della linguistica storica*, 1 (1949).94.

[73] *Esquisse d'une histoire de la langue latine*, 239 ff. and *passim*, Bloomfield, *Language*, 298.

condition. On the other hand, the persistence of alien linguistic groups means the continuation of a more primitive cultural state, which we must call prehistoric.

9. To sum up, to the extent that linguistics allow us to sound the depths of the human mind, it also permits us to trace back the course of prehistory until we reach the origins of man himself. With the aid of known languages we seek to determine what kind of language was used by the bearers of the cultures whose material remains are relatively near to us. On occasions the preservation of certain languages puts into our hands instruments of a very particular utility, as happens with Basque, which may very well be the survival of languages spoken in Western Europe in the Upper Paleolithic age.[74] The continuity *in situ* of anthropological characteristics would constitute a proof in favor of this survival, although we must make many reservations. It is certain that the Basque-Caucasian and Basque-Hamitic parallels refer to a very remote cultural world, in which metals were apparently lacking.[75] Certain conceptions reflected in Basque could be prehistoric, such as the comparison made by A. Pariente[76] between the Basque *erditu* and Lat. *parere* (a primitive verb, in face of *partire*, formed from *par-tis*), or the etymology established by me for the Basque *ezker* < **esku-erdi* 'half hand,' which is a relic of the way of designating twin organs in the Finno-Ugric, surviving also in Irish.[77] By submerging ourselves in a primitive linguistic world, we may also attempt cultural comparisons on a large scale basing ourselves on Basque.[78]

A new linguistic world has been discovered thanks largely to the efforts of Italian scholars. The secret of the old pre-Indo-European Mediterranean is partially revealed by linguistic remains collected by J. Jud, V. Bertoldi, F. Ribezzo, G. Devoto, G. Alessio, and J. Hubschmid. The prehistorian O. Menghin has constructed, at least provisionally, a general archeological and cultural framework. A critical collection of these materials is still needed, dispersed as they are, often in reviews difficult of access.[79] The weak point in these theories

[74] P. Fouché, *Emerita*, Suppl. to vol. 5, 70; L. de Hoyos Sainz, *Actas y memorias de la Soc. Esp. de antrop., etnol. y prehistoria*, 22 (1947), 69; Uhlenbeck, *Lingua*, 1.60f.; Lafon, *Eusko-Jakintza*, 1 (1947).35; Tovar, *Anales de Arqueología y Etnología*, 8 (Univ. de Cuyo, 1947).70; Caro Baroja, *Los pueblos de España* (Barcelona, 1946), 75; Pokorny, *Keltologie*, 133. On the general question of the permissibility of parallel deductions from prehistory and ethnology, W. Koppers in the work *Kultur und Sprache* (Vienna, 1952).31ff., R. Pittioni, *Archaeol. Austriaca*, Heft 14, 1954, 78ff.

[75] K. Bouda, *Baskisch-kaukasische Etymologien*, Heidelberg 1949, and other works; P. Kretschmer, *Glotta*, 32 (1952).1ff.

[76] *Emerita*, 14.81 n.

[77] Tovar, *Bol. de la R. Soc. Vascongada*, 7 (1951).453ff.583; *Zeitschrift für celtische Philologie*, 24.198ff. On the facts in Irish and Finno-Ugrian see Pokorny and E. Lewy, *Zeitschrift für celt. Philol.* 16.240f. and K. Bouda Der Dual des Obugrischen, 41f. (reprinted from the *Journal de la Soc. finno-ougrienne*, 47, 1934.)

[78] We are preparing an etymological Dictionary of Basque, and it is to be hoped that it will bring considerable material for this cultural comparison.

[79] Unfortunately a critical work on this subject is lacking, and we are obliged to resort to the most dispersed articles: a recent and very clear resumé is given by F. Ribezzo, *Arch. glottol. ital.*, 35.46–64.

is that they are difficult to relate to a concrete linguistic world, that is to say, to a language which we know sufficiently well. We must expect much from Caucasian linguistics and from the study of North-African languages. In addition, a classification of the Basque vocabulary from the point of view of its etymological relations would be very profitable.

The possibility of seeing strong coincidences between Indo-European languages and certain of their neighbors,[80] especially in interchanges of morphological elements, such as are found in more primitive cultural worlds like native America, may guide us in the study of the conditions under which the Indo-European invasions took place. Thus we have *-ko* forming adjectives, diminutives and derivatives resembling a similar process in Indo-European in Etruscan, Basque, Eteocypriote etc. The existence of a mixture of languages in those remote times gives us the key to an explanation of the formation of Indo-European when there reigned in central and oriental Europe in the 3rd millenium a culture which was still pacific and relatively uniform.[81]

10. Within the theme of prehistoric linguistics we must not omit an allusion to the origin of language, long a vexed question for philosophers and speculative linguists, and later expelled from scientific consideration by positivist sobriety.[82]

In the withdrawal of comparative linguistics to its firm positions in Indo-European (and other well defined fields) this problem was abandoned, and today, quite naturally, it is more a question for psychologists and philosophers of language[83] than for linguists.

However, we have here to make a reference to certain linguistic points of view on the theme of the origin of language. The theorist of Soviet linguistics for a quarter of a century, N. Marr,[84] established a "stadial" theory, as a not very logical consequence of the dialectical materialism of history. According to this, languages belong to four types, which correspond to equivalent stages of society: a primary type, to which belong Chinese and certain African languages; a secondary one, embracing Finno-Ugric and Turco-Mongolian; a tertiary one, which comprises Japhetic and Hamitic, corresponding to feudal society; a fourth type, Indo-European and Semitic, representing what would correspond in linguistics to a capitalist society. Finally, a universal language would be that of the future Communist society.

[80] In an attempt to develop this idea I wrote an article for the *Arch. glottol. ital.*, on the conception of "peri-Indo-European" by Devoto. From this point of view the first steps of P. Kretschmer to create a "Protindogermanisch" (*Glotta*, 14.300ff., 28.231ff., 30.84ff.) are too influenced by the old ghost of the "genealogical tree."

[81] Bosch-Gimpera, *Etudes Celtiques*, 5.353ff.; Pisani, *Allgemeine Sprachwissenschaft* . . . , 77ff.

[82] Some references in *Emerita*, 9.163, 18.226ff.

[83] G. Révész, *Ursprung und Vorgeschichte der Sprache*, Berne, 1946.

[84] A summary of the theories of Marr in H. Rubenstein, *Language*, 27.283 and W. K. Matthews, *Archivum linguisticum*, 2.1ff., 97ff., especially 7ff. The doctrines of Marr were not so new, as is easily proved by comparison with those of Max Müller as set out by G. Vidossi (in R. Biasutti, *Le razze e i popoli della terra*, 1.341).

Monogenist considerations[85] complicate the motivation of this doctrine, which is in no way scientific.

A new theory of language in relation to cultural stages, without considerations of a political nature or being comprehended within historical dialectics, has been formulated by G. Révész,[86] with influences of the Vienna school and modern psychology. This scholar traces six stages in a vast scheme, from the most elementary means of communication in the animal species to the two most elevated, "prehistoric" and historic. The fields established by him for these two stages ("invitations to work or to take a position," "an expression of psychic conditions or external situations," "questions") are characterized in both stages by the limitation in the prehistoric stage to the imperative, indicative and interrogative moods, and by the greater importance in it of gesture. Children also remain at a stage resembling the prehistoric until their third or fourth year. Beneath both, in the fourth stage, is "primitive Man," with a language limited to the imperative, the vocative, the locative, the *deixis*, shouts and gestures being of great importance.

From the point of view of the linguist, the vista of millenia of prehistory makes the problem of the origin of language insoluble. The most we can do is to establish an archaic stage of languages which would even have certain common characteristics. W. Schmidt[87] makes bold to define such characteristics from the phonetical point of view, and J. van Ginneken,[88] on his part, reconstructs a complicated primitive phonetical type on the basis of lateral consonants (and clicks of the tongue) found in Caucasian languages (and in Hottentot), and he supposes that originally vowels did not exist. Moreover he went so far as to suppose paradoxically that language is as young as writing.

With other objectives, but influenced by Father Schmidt, A. Sommerfelt[89] had studied an Australian language and believed he could indicate the primitive characteristics of a language corresponding to the remote past of humanity. J. W. Entwistle[90] accepted many of the results obtained by these new researches.

[85] Rubenstein, *loc. cit.*, 282f.

[86] *Ursprung*, 242f.

[87] *Die Sprachfamilien und Sprachenkreise der Erde*, 500ff.; idem in F. Kern, *Historia mundi*, 1.440. An exposition of the ideas of W. Schmidt and W. Koppers is given by G. Vidossi, *loc. cit.*, 342ff. That a certain correlation between types of culture and languages exist, seems beyond doubt. The difficulty is to determine wherein lie the corresponding characteristics of language. Cf. C. Lévi-Strauss in his introduction to the *Results*, 10.

[88] La réconstruction typologique des langues archaïques de l'humanité, *Verh. kon. Ned. Akad. Wetensch.*, Afd. Lett. 44. Amsterdam 1939, and the criticisms by M. Cohen and G. Guillaume, *BSL* 40 (1939).10ff., 14ff. and G. Deeters, *IF*, 60.100ff. The paradoxical opinion that writing preceded the word, sustained by Ginneken, is followed by Hjelmslev, *Prolegomena*, 67, who, in his turn, follows B. Russell.

[89] *La langue et la Societé. Caractères sociaux d'une langue de type archaïque*, Oslo, 1938. Criticism by M. Cohen, *BSL*, 39.12f.

[90] *Arch. linguisticum*, 1.117ff. This writer seems to accept an archaic type of language, which could be reconstructed on the Australian tongue studied by Sommerfelt or on Bantu as regarded by Madan and Doke. A "pre-grammar" would be assigned to this primitive

We leave the vexed question of the monogenesis or polygenesis of language in the zone of *ignorabimus*. It is curious that W. Schmidt, a cultural monogenist, maintains a position of prudent agnosticism as regards the origin of language,[91] and does not go so far as A. Trombetti, who spent his life on the cause of the monogenesis of language.

When did Man learn to speak? There have been numerous and ingenious speculations, founded on anatomical reasons. For some (Böklen) only at the Mustérian period, for others (O. Hauser) at the Aurignacian. Yet others estimate that Neanderthal man knew how to speak, but only in an undeveloped manner (Walkhoff and Heilborn). Others believe it was certainly before the discovery of fire, since such an element requires a collaboration which necessarily supposes communication (O. Peschel).[92]

In this question the most correct position, psychologically, is that of Goethe in his dialogue with Herder, when he affirms that the origin of language and the origin of Man are undistinguishable.[93] This being true, may we affirm that everything can be expressed in any language[94]? May we assert that the first man who spoke expressed himself in a language humanly similar to the rest? We believe so, since language is "formally complete," as E. Sapir has said, and it is either not language or it is symbolic. This symbolism comes from the work of "legislation," and in this Plato saw the mystery of language: *ouk ára pantòs andròs ónoma thésthai estín, allá tinos onomatourgoũ* (*Cratyl.* 389a).[95]

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type of language. But what is an "archaic" type of language still remains very problematic; see e.g. the criticism of E. Lewy and J. Lohmann (*Lexis*, 3.250 and 258) against the supposed primitiveness of the Aranta, the Australian language about which scholars were making theories.

[91] *Sprachfamilien*, 23ff.; *Historia mundi*, 1.440.

[92] I find all these data in the otherwise indefensible book of E. Böklen, *Die Entstehung der Sprache im Lichte des Mythos*, 1922, 176ff. and 186. Cf. W. Schmidt, *Sprachfamilien*, 12ff.; Oakley in *An Appraisal of Anthropology Today*, 264ff.

[93] Similar thoughts expressed W. von Humboldt and E. Renan; see Révész, *Ursprung*, 18 n. The same author adheres to this position. Compare too K. Ulmer, *Lexis*, 2.263ff.; A. Schiaffini, *Momenti di storia della lingua italiana*, 1953, 156ff.

[94] E. Sapir, *Selected Writings*, 153. The formula "formal completeness" of every language seems to us very exact. Men always have a language, says the scholar. Cf. E. H. Lenneberg, *Language*, 29.467; W. Schmidt, *Historia mundi*, 1.439, and the recent studies of Vaast van Bulck and P. Schebesta (*Kultur und Sprache*, 365ff., 426ff.); especially this last author gives an affirmative answer to the question of the Pygmees having their own language.

[95] Amongst modern linguists, G. Devoto has stressed the value of language as an "institution," cf. *I fondamenti della storia linguistica*, Firenze, 1951, 26ff.—I wish here to thank Mr. D. Webborn, who kindly helped me edit the present paper in English.

COMMENT RECONSTRUIRE LE CHINOIS ARCHAÏQUE

André G. Haudricourt

La linguistique chinoise se présente sous un aspect analogue à la linguistique romane: aux langues romanes correspondent les langues mandarines, cantonaise, Wou (du Fleuve bleu inférieur) et Min (du Fou-kien); au latin médiéval, langue écrite des pays germaniques, hongrois et slaves occidentaux correspond le chinois classique, langue écrite de la Corée, du Japon et du Vietnam; aux anciens emprunts latins du germanique, de l'albanais, du basque et du berbère correspondent les anciens emprunts chinois du miao, du thai, du vietnamien et du khmer.[1]

Deux différences importantes à signaler: le latin, langue flexionnelle, se classe immédiatement avec le germanique le slave et l'albanais dans la famille indoeuropéenne à laquelle n'appartiennent ni le basque, ni le berbère. Le chinois, langue à mots invariables, est plus difficile à classer: il appartient au groupe tibéto-birman, mais est-il aussi proche du thai qu'on l'a longtemps cru? La parenté dans ce type de langue ne peut se fonder que sur le vocabulaire, et l'importance des emprunts chinois a été souvent méconnue. D'autre part on a admis l'existence de langues mixtes:[2] lorsqu'une langue *C* présente une partie de son vocabulaire en commun avec la langue *A*, et une autre partie en commun avec la langue *B* on disait que la langue *C* était issue d'un mélange ancien d'un dialecte de *A* avec un dialecte de *B*.

Il semble maintenant que l'hypothèse la plus correcte est de considérer que les langues *A*, *B*, *C*, font partie de la même famille de langue. On aurait à faire, en Extrême-Orient, à une grande famille, par enchaînement, tibétobirman et chinois étant apparentés au miao et à l'austroasiatique, ceux-ci au thai, ce dernier au malayopolynésien.

Une seconde différence réside dans le mode d'écriture: le latin écrit alphabétiquement a gardé au cours des âges la physionomie de ses mots, les emprunts anciens ont suivi l'évolution phonique considérable des langues européennes et sont très usés par rapport aux emprunts récents qui ont conservé la graphie latine. Le chinois, écrit idéographiquement, est sans défense contre l'évolution phonique. La langue de civilisation parlée varie donc de siècle en siècle avec la situation de la capitale et le degré d'évolution phonique de la langue. En fait, la capitale étant située au nord, dans une région qui a subi au cours de l'histoire des invasions de Turcs, Mongols, Toungouzes et Mantchous qui fondèrent des dynasties, puis s'assimilèrent après une période de bilinguisme, la langue de la capitale a évolué beaucoup plus rapidement que les langues du sud de la Chine et de l'Indochine, les prononciations vulgaires des mots chinois des couches

[1] La meilleure mise au point récente est l'ouvrage de R. A. D. Forrest, *The Chinese Language*, 352 p.

[2] Par exemple H. Maspero lorsqu'il déclarait: «le préannamite est né de la fusion d'un dialecte mon-khmer, d'un dialecte thai, et peut être même d'une troisième langue encore inconnue.» *BEFEO*, XII, p. 117.

d'emprunt les plus anciennes sont mieux conservées que les emprunts savants les plus récents représentés par les lectures officielles des caractères idéographiques. Exceptionellement en Europe, on trouve des cas analogues par exemple en allemand où *Kaiser* est plus ancien et mieux conservé que *Cäsar*, en ce qui concerne la première syllabe.

Pour reconstituer l'ancien chinois nous disposons d'abord des ressources de la linguistique comparative: comparaison des dialectes chinois entre eux, utilisation des anciens emprunts chinois des autres langues, accord de ces restitutions avec les langues voisines apparentées généalogiquement.

La majorité des caractères d'écriture sont des phonogrammes, c'est-à-dire qu'ils fonctionnent comme des rébus. Ainsi l'idéogramme du «boa» *pa* est employé comme phonogramme dans les caractères qui écrivent les mots: *pa* «saisir», *pa* «cible», *pa* «herse», *pa* «cicatrice», etc. Mais l'écriture remonte à près de deux millénaires avant notre ère, et certains mots dont la prononciation était analogue à cette époque sont très différents maintenant. On aurait des indications précieuses, si nous pouvions à coup sûr distinguer phonogramme et idéogramme, ce qui n'est pas le cas. Ainsi le mot *fei* «gras» s'écrit avec un caractère contenant l'idéogramme «boa»; il est difficile de savoir si celui-ci indique l'idée de grosseur que rappelle ce serpent, ou la prononciation d'un mot débutant par une bilabiale suivie d'une voyelle ouverte, ou les deux à la fois.

C'est seulement à partir du III[e] siècle de notre ère que l'on aurait commencé à comparer la prononciation des caractères entre eux au moyen du procédé appelé *fan-ts'ie*: la prononciation d'un caractère est indiquée au moyen de deux autres, l'un ayant initiale identique, l'autre ayant finale identique. Par exemple la prononciation de *pa* est indiquée par les deux caractères: *päk* et *ka*. En se basant sur les *fan-ts'ie*, sur les prononciations japonaises et vietnamiennes des caractères, et surtout sur un dictionnaire de rimes établi en 601, mais dont on ne possédait jusqu'à présent que l'édition de 1008,[3] H. Maspero et B. Karlgren ont tenté de reconstituer la prononciation de la langue chinoise de l'époque des T'ang. Maspero et Karlgren n'étaient pas d'accord sur tous les points, mais c'est la reconstitution de Karlgren qui est la plus poussée, la plus systématique, et qui a été adoptée par la majorité des sinologues.[4] De plus, en partant de celle-ci et en se servant les phonogrammes et des rimes du Livre des Odes, Karlgren a également tenté de reconstituer la prononciation à l'époque de Confucius, celle du chinois de l'époque des Tcheou.[5] Les deux reconstructions de Karlgren sont solidaires et nous en critiquerons successivement les initiales et les finales.

[3] Une édition de 706 a été retrouvée ces dernières années à Pékin; un article de Wang Lien-tseng sur ce sujet est à l'impression dans le *T'oung pao*.

[4] Les deux ouvrages de H. Maspero sont. Etudes sur la phonétique historique de la langue annamite: les initiales, *BEFEO*, XII, 127 p. (1916) et Le dialecte de Tch'ang-ngan sous les T'ang, *BEFEO*, XX, 124 p. (1920); ceux de B. Karlgren sur le même sujet: *Etudes sur la phonologie chinoise* (parues de 1915 à 1926 dans *Archives d'études orientales*), et *Analytic Dictionary of Chinese and Sino-Japanese* (1923), 436 p. (Les caractères y sont numérotés, nous y renvoyons désormais sous le signe AA. . . .).

[5] La reconstitution de tous les caractères des textes de cette époque se trouve dans:

Maspero et Karlgren sont d'accord pour restituer en ancien chinois trois séries d'occlusives, une série sourde ordinaire: *p t k*, une série sourde aspirée: *ph th kh*, et une troisième série, sur la restitution de laquelle ils ne sont pas d'accord. Pour Maspero il s'agit de sonores ordinaires: *b d g*; pour Karlgren il s'agit de sonores aspirées: *bh dh gh*.

Les deux premières séries sont conservées dans toutes les langues. La troisième n'est restée distincte que dans les dialectes Wou sous la forme d'occlusive, à aspiration sonore. Maspero a montré, au moyen des transcriptions chinoises des mots sanscrits, que les occlusives sonores ne sont devenues aspirées qu'à la fin des T'ang (VIII^{e}–X^{e} siècles), et que pour les périodes plus anciennes, il faut restituer une occlusive sonore ordinaire.[6] Le fait que des occlusives sourdes aspirées actuelles proviennent d'anciennes occlusives sonores se retrouve en Indochine en thai[7] et en karen[8] où selon les dialectes les anciennes sonores assourdies sont aspirées ou non.

On pourrait croire que la divergence entre Maspero et Karlgren soit négligeable; de toute façon dans le faisceau de corrélations

p	t	k
ph	th	kh
b(h)	d(h)	g(h)

seule la sonorité est pertinente pour la troisième série, et il ne s'agirait que d'une différence d'appréciation sur la réalisation du phonème.

En réalité le fait est très important dans la reconstruction de Karlgren, car celui-ci, phonologue avant la lettre, a considéré l'absence de sonores ordinaires dans son tableau comme des "cases vides" à remplir.[9] Il restitue pour l'époque des Tcheou le système équilibré suivant:

p	t	k
ph	th	kh
b	d	g
bh	dh	gh

Il admet qu'à l'époque des T'ang, *b d g* étaient amuis et *bh dh gh* conservés.[10]

Grammata serica, Script and Phonetics in Chinese and Sino-Japanese, *BMFEA*, XII, 471 p. (1940). Il y a aussi une numérotation à laquelle nous renvoyons sous le signe GS. . . . .

[6] Le dialecte de Tch'ang-ngan, *BEFEO*, XX, page 27.

[7] Ce qui est prouvé par l'utilisation d'alphabet indien où on avait le choix entre sonores aspirées et non aspirées. Voir Les phonèmes et le vocabulaire du thai commun, *JA*, 236, p. 197–238 (1948); Voir aussi La conservation de la sonorité des occlusives du thai commun dans le dialecte de Cao-bang, *Actes du XXI^{e} Congrès des Orientalistes*, Paris 1949, pages 251–252.

[8] Restitution du karen commun, *BSL*, 42, p. 103–111; A propos de la restitution du karen commun, *BSL*, 49, p. 129–132.

[9] Sur le remplissage des «cases vides», A. Martinet, Rôle de la corrélation dans la phonologie diachronique, *TCLP* 8, p. 273; et Function, Structure and Sound Change, *Word* 8.1–32 (1952).

[10] On notera déjà l'invraisemblance phonétique qu'une sonore aspirée soit plus solide qu'une sonore non aspirée.

La restitution de cette nouvelle série de sonores est fondée sur le fait que des phonogrammes dépourvus d'occlusives initiales sont employés pour écrire des mots à occlusives initiales par exemple:[11]

i̯ʷong	pour écrire	*dhung thung zi̯ʷong*	AA 264
i̯uen	pour écrire	*ki̯uen*	AA 292
ji̯ou	pour écrire	*ghji̯ou zuâi*	AA 249
ji̯âm	pour écrire	*dhâm thâm*	AA 239
i	pour écrire	*d'hi dhi si thâi*	AA 184

Inversement des caractères représentant des mots à occlusive initiale sont employés comme phonogrammes pour des mots sans occlusive initiale, exemple:

kuk	pour écrire	*i̯ʷok i̯u'*	AA 425
tâ	pour écrire	*ti̯a ie̯*	AA 1006

Karlgren en conclut[12] que cette consonne «zéro» indique la disparition d'une occlusive sonore *g* ou *d*, et qu'il faut restituer dans les exemples précédents, pour une période antérieure aux T'ang:

di̯ʷong	pour écrire	*dhung thung*	AA 264
gi̯uĕn	pour écrire	*ki̯uĕn*	AA 292
etc.			

En ce qui concerne les occlusives affriquées, Karlgren déclare que *z* provient de *dz* et *ź* de *dź*, ce qui est vraisemblable, mais il en déduit l'existence de *dz*, *dź* sonores non-aspirées, phonèmes distincts des *dzh dźh*, et rétablit des anciens *z ź* à la place de la consonne «zéro» de certains phonogrammes.

Ainsi on aurait à l'époque T'ang:

i̯ang	pour écrire	*zi̯ang*	AA 211
i̯āk	pour écrire	*tsi̯āk i̯a'*	AA 187

mais à une époque antérieure:

zi̯ang	pour écrire	*dzi̯ang*	AA 211
zi̯āk	pour écrire	*tsi̯āk zi̯a'*	AA 187

Pour contester avec Maspero l'ancienneté des *bh dh gh dzh dźh* . . . il faut critiquer l'ensemble de la construction.

Karlgren a opéré avec rigueur en se fondant sur la régularité absolue des lois phonétiques et le remplissage de case structural comme si sur deux ou trois millénaires la langue chinoise avait eu un développement linéaire libre de toute influence dialectale.

[11] Notations de Karlgren, en remplaçant le signe d'aspiration par *h*. La notation du pékinois est faite selon la transcription française: P. Demiéville, *Matériaux pour l'enseignement élémentaire du chinois*, Paris 1953, 86-75 p. L'apostrophe, ', indique dans les restitutions le ton oblique: '(*chang*), (*k'iu*)', mais comme en vietnamien nous suivons l'orthographe officielle (ainsi Emeneau dans *Studies in Vietnamese Grammar* [v. *Word* 8.287–290]), le ' indique la «barke», c'est-à-dire la série moyenne; *u'* = *u*, *o'* = *ɔ*. Rappelons qu'en vietnamien *b* et *đ* sont des occlusives préglottalisées [v. *B.S.L.* 46.1.172–182] et *g*, *d* des spirantes.

[12] *Analytic Dictionary* . . . . pages 18–27.

Examinons d'abord la distinction entre affriquée et spirante sonores. Se fondant sur les initiales des dictionnaires, Karlgren distingue *dzh* et *z*, *dźh* et *ź*, mais ne connaît pas de spirante cacuminale sonore, seulement *ʤh*. Voyons le traitement de cette dernière initiale en pékinois, cantonais et sino-vietnamien:

N° de AA		sino-vietn.	canton.	pékin.
1096	*ʤhai*	*sài*	*ṣāi*	*tch'ai*
1024	*ʤhai*	*sài*	*tshāi*	*tch'ai*
1170	*ʤham*	*sàm*	*tshām*	*tch'an*
1259	*ʤhan*	*sàn*	*ṣān*	*tch'an, tch'ouan*
1062f	*ʤhi̯ang*	*sàng*	*tshōng*	*tch'ouang*
386	*ʤhi̯əm*	*sâ'm*	*ṣăm*	*tch'en*
1088	*ʤhi̯əu*	*sâ'u*	*ṣău*	*tch'eou*
1114	*ʤhi̯ung*	*sùng*	*ṣung*	*tch'ong*
834	*ʤhwan'*	*soạn*	*tsān*	*tchouan*
877	*'ʤhi*	*sĭ*	*ṣi*	*che*
889	*ʤhi'*	*sụ*	*ṣi*	*che*

Nous constatons que le traitement est régulier en vietnamien où nous avons une spirante, presque régulier en pékinois où nous avons une affriquée (aspirée au premier ton p'ing, non-aspirée aux autres tons, ce qui est la règle) sauf deux exceptions, mais en cantonais nous avons deux traitements comme s'il avait existé en ancien chinois un *ʒ* distinct du *ʤh*. Or il est évident ici que l'opposition phonologique des deux premiers mots: *sai tshai* ne suffit pas à prouver que les deux initiales ont de tout temps été distinctes en chinois. Les documents nous enseignent que cette distinction n'existait pas à l'époque T'ang, et que nous avons simplement à faire à la superposition d'un traitement cantonais indigène: *ʤh* > *ʂ* et d'un traitement des capitales du centre ou du nord: *ʤh* > *tsh*.

A-t-on le droit de faire remonter la distinction *dźh—ź* de l'époque T'ang à une époque antérieure? On peut en douter car cette distinction n'est pas respectée par les dialectes actuels, comme on peut s'en rendre compte par la liste suivante:

856, b, d	*ź̯iang*	*thu'ò'ng*	*ṣōng*	*tch'ang*
857 b	*ź̯iang*	*thu'ò'ng*	*ṣōng*	*tch'ang*
1196	*ź̯iĕn*	*thân*	*ṣăn*	*tch'en*
1203	*ź̯i̯əng*	*thù'a, thẳng*	*ṣing*	*tch'eng*
1144g	*ź̯iuen*	*thuâ'n*	*ṣun*	*tch'ouen, chouen*
1267	*źwi̯ę*	*thùy*	*ṣui*	*tch'ouei*
1247 h i	*ź̯i̯u*	*thu*	*ṣu*	*chou*
1129	*ź̯i̯a*	*xā*	*ṣe*	*chō*
228 b, 1187 r	*ź̯i̯ʷo*	*thụ*	*ṣū*	*chou*
897, 1265 n	*ź̯i̯əu*	*thụ*	*ṣau*	*cheou*
1220	*ź̯i̯ək*	*thục*	*tsik*	*tche*
860 d	*ź̯i̯ak*	*thuọc*	*tsōk*	*chao, chouo*
1207	*dźhi̯əng*	*thù'a*	*ṣing*	*tch'eng*
232 d	*dźhi̯ʷān*	*thuyê'n*	*ṣūn*	*tch'ouan*
868 e	*dźhi̯ĕn*	*thầ'n*	*ṣan*	*chen*
865	*dźhi̯a*	*xạ*	*ṣe*	*chō*
1204	*dźhi̯uĕn*	*thuận*	*ṣun*	*chouen*
862	*dźhi̯ăt*	*thiệt*	*ṣit*	*che*
445	*dźhi̯ĕt*	*thục, thât*	*ṣat*	*che*
891	*dźhi̯ək*	*thục*	*ṣik*	*che*
632 c	*dźhi̯əng*	*thang*	*ṣing*	*cheng*

Il ressort de ce tableau que le pékinois traite le *ź* exactement de la même façon que le *dźh*. On ne peut pas dire que le traitement pékinois représente un ancien *dź* distinct de *dźh*, puisqu'il présente le même traitement aspiré au ton p'ing que les autres occlusives que Karlgren restitue comme *bh dh gh*.

Le sinovietnamien indique le même traitement que le cantonais: *dźh* a été traité comme la sonore de *ś*, car en sinoviétnamien *ś* donne *th* tandis que *tś* et *tśh* donnent respectivement *ch* et *x*.[13] Maspero avait indiqué[14] qu'un auteur du VII[e] siècle, Yen Tche-t'ouei, déclare que les gens du Midi confondent *źi̯äk* (AA 883) et *dźhi̯äk* (AA 865), *źi̯ę* (AA 890) et *dźhi̯ę* (GS 867 f). Le même auteur indique que les gens du Midi confondent *dzhien* (AA 1072 e) et *zi̯än* (AA 235 d), c'est ce que l'on constate en cantonais. Exemples:

211 i j	*zi̯ang*	*ts'öng*	*siang*
810 c	*zi*	*ts'i*	*ts'e*
787	*zi̯ang*	*tsöng*	*siang*
811	*zi'*	*tsi*	*se*
253 k	*zi̯əu'*	*tsau*	*sieou*
775, 779	*zi̯äk*	*tsik*	*si*
781	*zi̯əp*	*tsap*	*si*

Pour expliquer tous ces faits, il faudrait faire une étude plus approfondie des différents dialectes, les examiner au point de vue de la géographie linguistique et faire une étude historique des différentes capitales et des centres administratifs dont le dialecte a pu avoir une influence directrice sur les autres. En tout cas, il est loin d'être évident que les oppositions affriquées sonores et spirantes sonores remontent à l'époque archaïque et qu'on puisse en tirer un argument sérieux pour une distinction: *dz—dzh*, *dź—dźh*.[15]

Examinons maintenant le second argument de Karlgren, les consonnes «zéro» des phonogrammes qui seraient d'anciens *b d g z*. On ne trouve aucune autre preuve du «remplissage de case» que la nécessité structurale. Si l'on s'addresse aux langues apparentées généalogiquement au chinois comme le groupe tibétobirman, on s'aperçoit qu'au *g-* de Karlgren correspond *w-*, *sw- rw-*, et qu'aux *d-*, *z-* correspond *y-* (en lushai *y-* devient régulièrement *z-*).[16]

Si l'on s'adresse aux langues qui ont fait des emprunts anciens au chinois, on trouve d'autres phonèmes que des occlusives. Ainsi le thai, le dioi[17] et les

[13] Il faut rectifier sur ce point Maspero: *Etudes sur la phonétique historique* . . ., les initiales, p. 26 et 53, qui utilise des lectures contestables de caractères rares.

[14] Le dialecte de Tch'ang-ngan, p. 15.

[15] S. E. Martin, The Phonemes of Ancient Chinese (Suppl. *JAOS* n° 16, 1953) signale, page 13, que *dźh* n'était pas compté au nombre des initiales à l'époque T'ang; on pourrait en conclure qu'il y avait encore confusion entre *ź* et *dźh*.

[16] P. K. Benedict, Archaic Chinese *g* and *d*, *HJAS*, XI, p. 197–206; mais il n'ose pas critiquer Karlgren et suppose que *d g* sont apparus en chinois; au contraire R. Shafer, The Vocalism of Sino-Tibetan, *JAOS*, 40, p. 302–337, id. 41, p. 18–31; Problems in Sino-Tibetan Phonetics, *JAOS*, 44, p. 137–143; The Initials of Sino-Tibetan, *JAOS*, 70, p. 96–103, ne tient pas compte des restitutions archaïques de Karlgren.

[17] Fang-Kwei Li, Some Old Chinese Loan-Words in the Tai Languages, *HJAS*, 8, p. 333–342; il critique nettement Karlgren.

langues austronésiennes du nord, lamet[18] et khmou,[19] ont emprunté les douze noms du cycle des années. Deux de ces noms ont les initiales qui nous intéressent:

AA	GS		sinovietn.	thai	dioi	khmou	lamet
283	*jɛ̆n* < 450	*djər*	*dâ'n*	*ñi*	*ñien*	*ñi*	*ñih*
258	*jəu* < 1096	*zjog*	*dậu*	*rau*	*ru*	*rau*	*rau*

Il est possible que le *ñ* thai soit un ancien *y*-, mais le *r*- est ancien en thai. Un emprunt ancien semble se rencontrer en vietnamien pour le mot:

			s-v.		vietnam.
1322 b	*iʷo* < 82 l	*dio*	*du*	«reste»	*thừ'a*

Or avec un ton de la série basse un *th* vietnamien ne peut représenter qu'un ancien *ź*.

Même la restitution de Karlgren pour l'époque tardive des T'ang ne semble pas exacte, car elle est contredite par le sino-vietnamien qui ne s'est individualisé qu'au X^{e} siècle. On peut s'en rendre compte par le tableau suivant:

AA		Sinovietn.	AA		Sinovietn.
1348 c	*jɟʷAt*	*việt*	1138 d e	*ɟʷāt*	*duyệt*
1345 b e	*jɟʷAn*	*viên*	232 b	*ɟʷān*	*duyê'n*
118 b	*jɟʷək*	*vục*	425 b c	*ɟʷok*	*dục*
524 y	*jʷi*	*vi*	1265 A g h	*ʷi*	*duy*
183	*ji*	*hĭ*	183 c	*i*	*di*
249	*jɟəu*	*hữ'u*	259 c	*ɟəu*	*du*
1267 e	*jɟəu*	*bu'u*	1327 g	*ɟu*	*dũ*
239	*jɟām*	*viêm*	247	*ɟām*	*diêm*

Si le *d*- vietnamien correspond aussi bien à un ancien *y*- (donc au *ɟ*- de Karlgren) qu'à un ancien *δ*-, par contre le *jɟ*- est évidemment inexact, et il faut avec les savants chinois[20] restituer un *γ*-.

Tous ces faits montrent que s'il faut restituer quelque chose à la place des consonnes «zéro», ce ne sont pas des occlusives mais plutot des spirantes sonores; le second argument de Karlgren s'évanouit et au lieu d'un système archaïque:

t	*th*	*dh*	*d*
tś	*tśh*	*dźh*	*dź*
k	*kh*	*gh*	*g*

il est plus vraisemblable de poser:

t	*th*	*d*	*δ, y*
tś	*tśh*	*dź*	*ź, r*
k	*kh*	*g*	*γ, w*

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[18] K. G. Izikowitz, *Lamet Hill Peasants in French Indochina*, Göteborg 1951, page 171.

[19] H. Roux, Les Tsa khmu, *BEFEO*, 27, p. 184.

[20] Yuen-ren Chao, Distinctions within Ancient Chinese, *HJAS*, 5, p. 223–227, et Lo Ch'ang-pei, Evidence for Amending B. Karlgren's Ancient Chinese *j* to *γ*, *HJAS*, 14, p. 285–290, mais il ne faut pas en conclure que ce *γ* remonte à un ancien *g* comme le fait G. A. Kennedy, Voiced Gutturals in Tangsic, *Language*, 28, p. 457–464.

Un autre cas de divergence entre Maspero et Karlgren est celui des groupes de consonnes à l'initiale.

Lorsqu'on rencontre avec le même phonogramme les caractères de mots en *l-* et de mots en *k-*, les deux savants sont d'accord pour estimer que *k-* représente un ancien *kl-*; pour Maspero il s'agit d'un mot en *l-* auquel on a ajouté un préfixe *k*; pour Karlgren au contraire ces mots en *l-* actuels sont d'anciens mots en *gl-*.

Maspero a eu l'avantage de s'appuyer sur le thai;[21] en effet l'étude des emprunts anciens au chinois permettra seule de résoudre le problème. Par exemple Karlgren considère le caractère (AA 574 z) *pâk* «éplucher» comme idéographique formé de (AA 574) *luk* «graver» et de (AA 975) *tâu* «couteau» tandis que Maspero le considère comme phonogramme et restitue *plɔk* en s'appuyant sur un mot thai commun *pluək* «écorce». Il semble ici que Maspero a eu tort car on trouve en thai commun *pɔk* «éplucher» et en vietnamien *bóc* «id.».[22] Le vietnamien a possédé des *kl-*, *pl-* assez tardivement, mais jusqu'à présent on n'a pas trouvé de mot d'origine chinoise avec ces initiales. Par contre le thai est plus instructif:

Thai commun	«changer»	*plien'*	AA	590 j	*pi̯ᵘăn'*	GS 178 o	*plian*
Thai commun	«abeille»	*'phrung*	AA	31 e	*phi̯ᵘong*	GS 1197 s	*phiung*
Thai commun	«nom de cycle»	*'plaw*	AA	1242	*'t'hi̯ɔu*	GS 1076	*thniôg*
Thai commun	«nom de cycle»	*'hmaw*	AA	602	*'maw*	GS 1114	*mlôg*

Pour le premier exemple Karlgren et Maspero sont d'accord. Le second dû à Maspero, et les deux derniers dûs à Li Fang-kwei[23] montrent à quel point les reconstructions de Karlgren sont peu fondées.

Il faut probablement distinguer les groupes en *-l-* et les groupes en *-r-*, car thai et vietnamien sont d'accord pour distinguer *r* et *l* dans les emprunts anciens de mots qui ont un *l* en chinois moderne ou en sinovietnamien. Exemples:

GS 1193	*li̯ung* > *li̯ᵘong*	dragon	*rô'ng*		
GS 185 f	*glân* > *lân*	enclos	*ràn*		
GS 185 i	*glian* > *lien*	former	*rèn*		
AA 552	*gli̯am* > *li̯ăm*	store	*rèm*		
AA 576	*luk*	poulie	*rọc*	(siamois)	*rɔk*
GS 1032	*li̯ôk* > *li̯uk*	six		(thai commun)	*hrŏk*

Lorsque *r* était précédé d'une occlusive, on a *s* en vietnamien, par exemple le nom de l'écureuil, *prɔk* en bahnar, khmou, siamois, devient *sóc* en vietnamien. Les mots chinois ont été munis d'un préfixe dans les cas suivants:

GS 928	*li̯ək*	force	*sứ'c*
AA 550	*lâp*	cire	*sáp*
AA 551	*lien*	lotus	*sen*

Une étude des *fan-ts'ie* antérieurs à l'époque des T'ang pourrait seule nous in-

[21] H. Maspero, Préfixe et dérivation en chinois archaïque, *MSL* 23, p. 313–327 (1930).

[22] C'est en groupant des rapprochements de ce genre que K. Wulff dans *Chinesisch und Tai* a cru pouvoir démontrer l'existence d'infixes en chinois et en thai.

[23] Li Fang-Kwei, op. cit., *HJAS*, 8, 333–342.

diquer si l'ancien chinois a distingué *l* et *r* comme la plupart des langues de la famille sino-tibétaine.[24]

Le thai commun distinguant vélaires *k g* et uvulaires *q G*,[25] certains emprunts chinois se présentent avec une uvulaire, tel *Gam* «or», *qang* «fonte de fer», et on peut se demander si le chinois archaïque n'a pas connu aussi cette distinction.

Les emprunts ne sont jamais une preuve décisive, mais ils donnent des indications précieuses que l'on ne doit pas négliger.

Les restitutions vocaliques de Karlgren soulèvent des critiques plus sérieuses.

On sait que le chinois moderne a une structure syllabique caractérisée par la possibilité d'un phonème médian, une semi-voyelle: *y*, *w* ou *ü* placée entre la consonne initiale et la finale vocalique. Au début de leur reconstruction, Maspero et Karlgren ont cru retrouver le *-w-* dans le *ho-k'eou* des tables de rimes et le *-y-* dans les «catégories» *teng*. Maspero décomposait *ü* en *w*+*y* et Karlgren en *y*+*w*. Karlgren distinguait entre *w* et *u*, *i̯* et *i* selon que le phonème faisait partie de la rime ou non. En 1915 Karlgren interprétait une différence de *teng* par une mouillure: *kji̯ən* (AA 384) contre *ki̯ən* (AA 385), alors qu'en 1923 il l'interprète par une différence de timbre: *ki̯ĕn* (AA 384) contre *ki̯ən* (AA 385), mais il ne persiste pas dans cette voie et n'hésite pas à faire remonter au chinois archaïque la majorité de ces phonèmes médians.

Au contraire Maspero explique leur genèse par des diphtonguaisons en s'appuyant sur les emprunts en thai (cf. note 24):

GS 627 a	*kliam* > *kiem*	«réunir»	thai commun:	*kɛm*
GS 361 a	*thien* > *thien*	«ciel»	thai commun:	*thɛn*

Le thai commun connaissait la diphtongue *ie*, donc nous avons bien l'évolution *ɛ* < *ie*. Les emprunts en vietnamien, quoique moins anciens, montrent les mêmes faits (voyez précédemment: «former» *ren*, «lotus» *sen*).

GS 625 a	*bhi̯ʷam* > *bhi̯ʷAm*	«tous»	thai commun:	*'brɔm*

Ici nous avons à la fois le passage de *ɔ* > *uo* > *wA* et le passage de *-r-* à *-y-* que nous avons déjà vu pour le nom de l'abeille.

Une autre origine des *-w-* pourrait être recherchée dans le dédoublement de labiovélaires en vélaires + *w*; en effet il semble que l'on n'ait pas remarqué qu'un certain nombre de finales de l'Analytic Dictionary n'existent que précédées de vélaires: *k*, *kh*, *g*, *x*, *ng*, tel: *-iʷei*, *-ʷAng*, *-i̯ʷäng*, *-ʷâk*, *-iʷet*, etc.

[24] Naturellement dans les ouvrages cités, Maspero considérait le thai comme apparenté généalogiquement au chinois, mais si avec P. K. Benedict (Thai, Kadai and Indonesian, A New Alignment in South-Eastern Asia, *American Anthropologist*, 44, 576–601) on pense qu'il s'agit d'emprunts, leur témoignage n'en est que plus probant.

[25] Maspero avait commencé à ne pas admettre en vietnamien d'emprunts chinois antérieurs au sinovietnamien; il en reconnait quelques-uns dans un court article: Quelques mots annamites d'origine chinoise *BEFEO*, 16, 3, p. 1–5 (1916), il en signale d'autres en 1920: Le dialecte de Tch'ang-ngan, pages 61 et 93 note 2. Depuis cette date aucun travail sur ce sujet n'a été publié.

Si l'on admet le point de vue de Maspero, la distinction de Karlgren entre *-w-* et *-u-* fondée sur la rime pourrait n'être qu'une distinction entre phonème labial et trait pertinent labial. En effet les deux finales en *-u-* distinctes dans le dictionnaire de rime de 706: *-uâi* et *-uən* sont en sinovietnamien: *-ôi* et *-ôn* (*-âi* et *-ən* donnent en sinovietnamien *-o'i* et *ăn*) tandis que les finales en *-u-* plus tardives: *-uâ*, *-i̯uən* et celles en *-w-*: *-ʷa*, *-ʷai* donnent en sinovietnamien des finales à phonème labial distinct: respectivement *-oa*, *uân*, *-oa*, *-oai*.

A côté du thai et du sinovietnamien que Maspero avait utilisés nous allons maintenant nous servir des anciens emprunts chinois en vietnamien proprement dit, qui, jusqu'ici, n'ont pratiquement pas été exploités (cf. note 25); moins anciens que ceux du thai, ils sont plus nombreux et permettent des séries démonstratives.

Examinons par exemple les emprunts terminés par une des diphtongues *ia*, *u'a*, *ua*, et comparons-les aux restitutions de Karlgren des mots correspondants:

Restitutions de Karlgren			vietnamien	
GS 23 f	*lia*	> *ljiɛ̯*	*lìa*	quitter
GS 2 ru	*ngia*	> *ngjiɛ̯*	*nghiã*	équité
GS 358 j	*tsiar*	> *tsi̯ɛ̯*	*tĩa*	pourpre
AA 890 b		*zi̯ɛ̯*	*thìa*	cuiller
			chìa	cure-oreille
GS 25 a	*bhia*	> *bhjiɛ̯*	*bìa*	écorce
GS 874 d	*pieg*	> *pjiɛ̯*	*bia*	stèle
GS 49 s'	*kio*	> *ki̯ʷo*	*cu'a*	scie
AA 579 j		*li̯ʷo*	*lù'a*	âne
GS 84 g	*tio*	> *ti̯ʷo*	*chú'a*	loger
GS 82 l	*dio*	> *i̯ʷo*	*thù'a*	reste
GS 60 i	*xio*	> *xi̯ʷo*	*hú'a*	permettre
GS 46 z	*dzhio*	> *dzhi̯ʷo*	*chữ'a*	soigner
GS 60 p	*ngio*	> *ngi̯ʷo*	*ngù'a*	empêcher
GS 102 h	*piwo*	> *pi̯u*	*búa*	marteau
GS 136 c	*pui*	> *pi̯u*	*búa* (*chợ'-*)	marché
GS 136 m	*bhiu*	> *bhi̯u*	*bùa*	amulette
GS 1001	*bhiug*	> *bhi̯əu*	*bụa* (*goá-*)	veuve
GS 101 f	*bhiwo*	> *bhi̯u*	*vùa*	aider
GS 129	*tiu*	> *tsi̯u*	*chúa*	seigneur
GS 129 d	*tiu*	> *tsi̯u*	*chua*	annoter
GS 1109 j	*miug*	> *mi̯u*	*mua*	se procurer
GS 1028 e	*siôg*	> *si̯əu*	*thua*	broder

La première finale correspond bien; la seconde également, à condition d'observer que le *u'* vietnamien rappelle le *i* par la position des lèvres et le *w* par celle de la langue, mais la troisième pose un problème.

Le parallélisme des trois finales s'observe en sinovietnamien où elles sont respectivement: *-i*, *-u'*, *-u*; il semble que Karlgren ait fait une erreur de chronologie en posant pour la troisième un stade plus jeune que pour les deux autres et je proposerai *ie*, *i̯ʷɔ*, *wo*.

Karlgren pourrait nous objecter que les mots chinois du vietnamien n'appartiennent pas au dialecte qu'il restitue, mais à un dialecte méridional, dit de Wou, auquel il attribue le plus ancien système japonais de lecture des caractères

chinois, le *Go-on*.[26] Autant il est naturel que ce dialecte, à l'époque T'ang, soit différent de celui de la capitale, autant il est difficile de comprendre pourquoi Karlgren n'en a pas tenu compte pour restituer le chinois archaïque; il maintient la finale *iu* pour la majorité des cas, même pour la période archaïque.

Une autre finale, dont le traitement sinovietnamien est *ô*, se retrouve dans de nombreux emprunts:

GS 75 e	*kho* > *khuo'*	*kho*	magasin
GS 49 u	*kho* > *'khuo*	*khó*	difficile
GS 60	*ngo* > *'nguo*	*ngọ*	nom d'un signe cyclique
GS 63	*tho* > *'thuo*	*thỏ*	lièvre
GS 801 b	*dhâg* > *dhuo'*	*đò*	bac
GS 53	*gho* > *'γuo*	*họ*	famille
GS 94 z	*no* > *'nuo*	*nỏ*	arbalète
GS 792 j	*sâg* > *suo'*	*tỏ*	informer
GS 69 l	*lo* > *luo*	*lò*	fourneau
GS 766 t	*'glâg* > *luo'*	*lõ*	saillant

Mais à côté de ce traitement en *o* ouvert, nous en avons en *a*:

GS 802 f	*mâg* > *muo'*	*m(a?)* tombeau		
GS 58	*ngo* > *'nguo*	cinq	(thai commun)	*'hnga*
GS 94 z	*no* > *'nuo*	*nỏ* arbalète	(thai commun)	*'hna* (khmer *sna*)
GS 60	*ngo* > *'nguo*	signe cyclique	(thai commun)	*sanga* (khmou *snga*)

Or la comparaison sino-tibétaine montre que cette finale provient d'un ancien *-a*,[27] il semble donc que l'on ait eu en chinois archaïque et jusque sous les Han (l'arbalète semble avoir été inventée et diffusée à cette époque) un *a*, puis que cet *a* se soit fermé progressivement, en passant par *o* ouvert au VI[e] et VII[e] siècles (emprunts vietnamiens), par *o* fermé au IX[e] siècle (sinovietnamien) pour aboutir à *u* en chinois moderne.

Ceci remet en question le reste du vocalisme: ainsi parce que *ma* «cheval» rime dans le livre des Odes avec des mots en *-o*, Maspero et Karlgren en ont conclu qu'il fallait restituer dans ce mot un *o* ouvert ou un *å*; mais puisqu'à cette époque les prétendus *-o* étaient des *-a*, il n'y a aucune raison de ne pas conserver la voyelle de *ma* «cheval», ce qui semble confirmé par le japonais *uma* «cheval» et le coréen *mal* «cheval». La consonne finale conservée par le coréen explique pourquoi le *a* s'est conservé dans ce mot au lieu de se fermer.

[26] Dans Grammata serica, pages 71–72, Karlgren reconnaît que sa reconstruction ne lui permet pas d'expliquer le dialecte ancien de Wou (Go est la prononciation japonaise de Wou), mais il en conclut que ce dialecte de Wou est un mélange. Est-il vraisemblable que le premier système de lecture chinois arrivé au Japon au VI[e] siècle ait été plus mélangé que le dialecte de la capitale chinoise au VIII[e] siècle? P. Demiéville déclare (*BSL*, 43, 2, 142): « L'hypothèse d'une multiplicité de dialectes de Wou pour expliquer les incohérences du Goon est elle aussi toute gratuite et des plus discutables; elle ne trouve aucun appui dans ce qu'on peut savoir par les sources japonaises des conditions historiques dans lesquelles s'est formé le Goon.»

[27] Exemple, tibétain *lnga* «cinq», birman *nga*.

Les comparaisons sinotibétaines montrent que certaines voyelles ont dû s'ouvrir, ainsi:

«trois» tibétain *gsum* GS 648 *səm* > *sâm*

Karlgren conserve la finale *-ək* pour toutes les périodes. Or nous avons un traitement différent en sinovietnamien:

GS 905 *tək* > *tək* *đắc* obtenir

et dans les emprunts plus anciens:

GS 919 k	*tək*	> *tək*	*đứ'c* vertu
GS 961 h'	*dhək*	> *dhək*	*đ̣'c* mâle
GS 904 c	*mək*	> *mək*	*mự'c* encre

Ce qui pourrait justifier une évolution: *u* > *ɯ(u')* > *ə* > *a*.

Les observations précédentes permettent d'améliorer l'équilibre du système vocalique du chinois archaïque pour lequel Karlgren avait restitué cinq voyelles postérieures (*u, ô, o, â*) contre aucune voyelle antérieure, en finale de mot.

En effet les voyelles antérieures du chinois archaïque ont été considérées par Karlgren comme obligatoirement suivies de consonnes. Cependant les emprunts vietnamiens confirment l'origine moyenne de l'une des rimes en *-i*:

GS 956	*ngɨəg* > *ngji*	*ngò'*	douter
GS 976 h	*dzɨəg* > *'zi*	*tợ'*	semblable
GS 963	*dɨəg* > *'ži*	*chợ'*	marché
GS 971	*ʤhɨəg* > *ʤhi*	*thờ'*	honorer
GS 972 j	*dzɨəg* > *zi*	*tờ'*	écrit
GS 974	*sɨəg* > *si*	*to'*	fil
GS 961 z	*dɨəg* > *ži*	*giờ'*	moment
GS 961 d'	*šɨəg* > *ši*	*thơ'*	poésie
GS 952 v	*ghɨəg* > *ghji*	*cờ'*	drapeau

Ce traitement coïncide avec celui qu'on trouve en thai commun pour trois noms de signe du cycle duodénaire:[28]

GS 937	*ghəg* > *'γâi*	*'gəu*
GS 964	*tɕɨəg* > *'tsi*	*'čəu*
GS 967	*dzɨəg* > *'zi*	*'səu*[29]

L'autre rime en *-i* était probablement un voyelle antérieure malgré la restitution de Karlgren:

GS 603	*kɨer* > *'kji*	*ghế'*	siège, table
GS 567	*mɨər* > *mjᵂi*	*mâ'y*[30]	sourcil
GS 561	*sɨər* > *si*	*thây*	cadavre

[28] Li Fang-Kwei, op. cit.

[29] La correspondance thai *s* pour chinois *z* est normale, le thai n'ayant pas encore de *z* qui est apparu ensuite provenant de *dr-*. Exemple: GS 686 *dɨəp* > *zɨəp*, thai comm. *sip* «dix».

[30] Le vietnamien *-ây* représente une ancienne diphtongue *ei*; exemple AA 776 *siei* devient *tây* «ouest».

Karlgren restitue un *-r* lorsque le phonogramme sert également à représenter une finale en *-n*. Le vietnamien ne permet pas la vérification puisque les anciens *-r* et *-l* de l'austroasiatique sont devenus en vietnamien *-y* ou zéro.

Dans certains cas l'emprunt chinois étant pourvu d'un *-y* (*-i*) exemple:

GS 2 q	*ngâ*	> *ngâ*	*ngài*	bombyx
GS 17 e	*mwâ*	> *muâ*	*mài*	broyer
GS 20	*ngwa*	> *ng^{w}a*	*ngói*	tuile
GS 3 q	*dia*	> *iɛ̨*	*dò'i*	changer
GS 1 u	*ghia*	> *ghjiɛ̨*	*cõ'i cũ'i*	chevaucher
GA 1 x	*kia*	> *kjiɛ̨*	*go$^{?}$'i gu$^{?}$'i*	confier

Il est possible que ce *-i* final represente la trace d'un *-l*, *-r* final que Karlgren n'a pas restitué.

Par contre, les finales en *-g*, dont Karlgren a émaillé ses reconstructions archaïques, posent un problème plus compliqué. Karlgren et Maspero ont constaté que dans les séries de mots écrits avec le même phonogramme les quatre tons[31] du chinois ancien ne sont pas également répartis: les tons *p'ing* et *chang* d'une part, le ton *jou* d'autre part ne se trouvent jamais dans la même série (ce qui n'est pas surprenant, le ton *jou* étant le seul ton possible des mots terminés en *-p*, *-t*, *-k*; les autres tons concernent les mots à finales vocaliques ou sonantes). Par contre le ton *k'iu* se rencontre indifféremment dans l'une ou l'autre des séries. Les mots à ton *k'iu* à phonogrammes à ton *jou* n'ont jamais de finales nasales, exemples:

AA 425	*ɟu'*	écrit avec le caractère	*kuk*
AA 187	*ɟa'*	écrit avec le caractère	*ɟăk*
AA 201	*$^{?}$i'*	écrit avec le caractère	*$^{?}$ɟĕt*

Karlgren et Maspero ont été d'accord pour admettre que dans ce cas les mots à ton *k'iu* ont dû avoir en chinois archaïque une occlusive finale, et ils rétablissent une occlusive sonore les trois mots cités deviennent: *iug iag $^{?}$id*.

Karlgren va plus loin: ayant constaté que dans les poésies les plus anciennes, le livre des Odes, les mots au ton *k'iu* appartenant aux séries à ton *jou* (ayant un *-g* restitué) riment parfaitement avec des mots au ton *k'iu* appartenant aux séries à tons *p'ing-chang*, il a généralisé la restitution de *-g* dans tous les mots de ces séries. Le résultat a été de faire disparaître de sa restitution du chinois archaïque toutes les finales en *-y* (*-i*) et en *-w*. Le nom du chat *miao*, qui semble si onomatopéique, devient *miog* (GS 1159 c)! Même au point de vue des rimes du Livre des Odes, le résultat n'est pas satisfaisant puisqu'il est obligé de faire rimer *-o* et *-âg* et de supposer une évolution dialectale dans le texte classique le plus archaïque.[32]

Les faits indochinois permettent de suggérer une autre hypothèse. La plus ancienne couche d'emprunt chinois en vietnamien nous montre une correspon-

[31] A l'époque T'ang, selon les dictionnaires de rimes, le chinois n'avait que quatre tons; l'assourdissement des sonores a dédoublé le nombre des tons à partir du IX^{e} siècle. Clair exposé de S. E. Martin dans *The Phonemes of Ancient Chinese*, pages 10–13.

[32] *Grammata serica*, page 31.

dance entre le ton chinois *k'iu* et les tons vietnamiens *hoi* et *nga*:

GS 1 x	*kia* > *kjię'*	*go*$^{?}$*'i*
GS 2 r u	*ngia* > *ngjię'*	*nghiã*
GS 766 t'	*glâg* > *luo'*	*lõ*
GS 792 j	*sâg* > *suo'*	*to*$^{?}$
GS 802 f	*mâg* > *muo'*	*ma*$^{?33}$

Or Maspero a montré depuis longtemps[34] que ces tons vietnamiens proviennent d'une spirante finale *-h* provenant de *-s* final.

Supposons un instant l'existence en chinois archaïque d'un suffixe *-s*. Ce suffixe pouvant s'ajouter à n'importe quel mot, il peut aussi bien s'ajouter à un mot à finale sonore (c'est à dire de la série *p'ing-chang*) qu'à un mot à finale sourde (c'est à dire de la série *jou*). Dans ce dernier cas les finales *-ks*, *-ts*, *-ps* sont devenues *-gs*, *-ds*, *-bs*, puis *-ys*, *-ws*, *-s*; il semble que les finales en *-ks* se soient vocalisées dès la période archaïque (en *-ws*, *-ys*) puisque dans le livre des Odes elles riment avec les finales en *-ws*, *-ys* (provenant de l'addition de *-s* aux finales *-y*, *-w*).

Au contraire les finales en *-ts* se sont maintenues plus longtemps. Un nom de signe duodénaire, donc faisant partie de la plus ancienne couche d'emprunt en thai (le seul de ces noms qui soit au ton *k'iu*), selon Karlgren: GS 531 *miwəd* > *mj*^{w}*ẓi*, se rencontre en thai, khmou, lamet sous la forme: *mot*. Selon notre hypothèse, il faudrait restituer *mwəts* ou *mots*. L'amuisement des finales *-ts* a dû se produire peu avant l'époque T'ang, car on constate à cette époque toute une série de finales qui n'existent qu'au ton *k'iu* et non aux tons *p'ing* et *chang* (Analytic Dictionary *-âi*, *-uâi*, *-i*^{w}*āi*, *-i*^{w}*ei*, etc.).

La valeur du suffixe *-s* reste à déterminer. A notre avis il ne faut pas employer la méthode trop «mentaliste» de Karlgren qui déclare: «We have the word pair *âk* "bad" and *âg* "to hate." Here the voiced final *-g* indicated a verb. But we also had the word pair *dhâk* "to measure" and *dhâg* "a measure." Here the voiced final indicated a noun.»

Il ne faut pas s'occuper d'abord de la «partie du discours» à laquelle appartiendrait le mot, selon sa traduction dans une langue européenne, mais de son sens réel; ainsi à mon avis les couples de mots précédents entrent dans un système de dérivation:

	âk mauvais	*âks* haïr
comme:	*xâu* bon	*xâus* aimer
	dâk mesurer	*dâks* mesure
comme:	*ṣi* envoyer	*ṣis* ambassadeur

Karlgren ne tenant plus compte des tons dans sa restitution du chinois archaïque confond «bon» et «aimer»: GS 1044 *xôg*, «envoyer» et «ambassadeur»: GS 975 *ṣlịəg*.[35]

[33] L'origine des tons vietnamiens, *Journal asiatique*, 241 (1953).
[34] Etudes sur la phonétique, *BEFEO*, 12, page 102.
[35] B. Karlgren, *The chinese language*, New York 1949, page 96.

LANGUAGE CONTACTS

HANS VOGT

Important trends in modern linguistics, which can be traced back to Saussure and Troubetzkoy in Europe, and to Sapir and Bloomfield in America, have strongly emphasized the descriptive techniques to be applied in the study of language. Accordingly, great efforts have been made to work out general and objective methods of description. It is nowadays generally admitted that languages can be studied in a scientific way and described independently of their historical development, and that such descriptions are prerequisite to historical studies, where one has to compare and connect successive stages of a language. In linguistic theory new terminologies have been proposed in which terms such as systems and codes, patterns and structures, play a great part. Much has been done to define these terms and to make use of them. Along with this, however, there has been in recent years a clearly observable shift of emphasis in discussions on theory and methods, a shift from the question: What is language? to questions of a less ambitious character: What does the linguist do with his data and how does he do it? In many important contributions to linguistics of the last years this pragmatic approach is unmistakable.

The linguist who works on a living language with data collected in the field by himself or by others can only handle a limited, smaller or larger, but finite corpus of texts.[1] He could limit himself to transcribing his data and to giving an arbitrarily arranged list of recorded utterances, but usually he will try to do more than this. He will analyze his data in order to isolate all the smallest units they are composed of. Subsequently, he will try to formulate the rules for the combinations of these units; perhaps not only rules which account for the combinations which actually occur in his data, but also more general rules which allow him—and everybody else—to predict combinations which might occur in future speech events, provided, of course, that the language remains identical with itself.[2] Generalizations of this kind always go beyond the immediately given data. The larger and more varied his corpus of texts, the greater the probability that his generalizations will prove correct, i.e. that his predictions will be confirmed by later experience. The ultimate test is the test of verification—the generalizations must therefore in principle be verifiable. If they are not, they fall outside

[1] It is perhaps not superfluous to insist on this point, however trivial it may seem. Knud Togeby says, for example in his *Structure immanente de la langue française*, Copenhagen 1951, that "l'objet d'une description immanente est . . . la langue considérée comme *un texte sans fin*" (p. 23), and later that "la première opération de la syntagmatique de la procédure immanente consiste à diviser *le texte infini* de la langue en ligne de l'expression et ligne du contenu" (p. 25, italics ours). These expressions are probably to be taken in the meaning of an indefinitely extensible text.

[2] For an interesting exchange of views on this question, see Charles F. Hockett, A Note on Structure (Review of de Goeje, By W.D. Preston, *IJAL* 14.1948), *IJAL* 14. 269–71 (1948), and W.D. Preston, Reply to Hockett, *IJAL* 15.133–35 (1949).

the scope of linguistics as a science. Many statements about language belong to this last type.

Grammarians and linguists of all times have generalized beyond their data; but what distinguishes linguists of our times from the older generations, is perhaps more than anything else our awareness of and curiosity about what we are doing when we generalize, and how and why we do it. The linguist's own behavior has become an object of research, just like language itself.

The fact that generalizations are made and often stand the test of verification would seem to imply some distributional regularity for the units of the data. The assumption that they can be verified by later experience if the corpus of texts were infinitely extensible must lead to notions such as those we mentioned, even though they are sometimes left rather undefined and vague.

The legitimacy of generalizations and the value of the corresponding statements about structure depend on the amount and the nature of the data with which the linguist works. His ultimate aim may be to describe languages as systems. However, since systems are not immediately given, but emerge as the result of the linguist's arrangement and interpretation of the chaotic data, he may easily be led to overestimate his proper role at the expense of the data. He may get the impression—and often rightly so—that too many data threaten to confuse and blur the great and simple lines of the structure he wants to unravel. The success of many brilliant phonemic and morphemic analyses is, I am afraid, due to the incompleteness of the data, just as the confusion of descriptions on more traditional lines is due to the very exhaustiveness of the treatment. It is sometimes said that the description of a language ought to be exhaustive, consistent and non-contradictory and as simple as possible. Exhaustiveness and simplicity are not easily reconcilable. Structuralists have a tendency to sacrifice to simplicity, whereas their predecessors in their search for completeness got lost in the details.

If we accept the statement that languages form systems whose structures can be studied and presented, we shall also have to admit that such a general statement represents a gross simplification. From all descriptions of languages it appears clearly that language is not equally structured in all its domains. There are highly and less highly structured domains, and domains which hardly exhibit structure at all. Structures are probably easiest to show in the domain of phonemics. Speakers belonging to the same linguistic community have, as a matter of fact, practically all the same phoneme inventory and the same phoneme combinations in their speech. As for morphemics the same holds only to some extent. Some parts of it are highly structured and uniformly so for all the speakers, but on many points there are noticeable individual differences in structure from one speaker to another, considerably more so than in the case of phonemics. The *passé simple* is part of the verbal system for many Frenchmen, but it is unknown in that of many others, so that it would be possible and admissible to posit two different verbal systems for French. Since these two systems overlap on so many points, it is, however, equally possible, and probably preferable, to consider both of them as subvarieties of a single more general system, characterized, among other things, by the specific role of the *passé simple*, and the corresponding

ambivalence of the *passé composé*. As concerns vocabulary real structures can be discovered only for parts of it.

In dealing with linguistic systems and the ways of describing them there are, however, two points I should like to draw attention to, because they are not often explicitly stated.

All languages we know change with time; all of them are, at each definite moment of their lives, in a state of flux. When we describe systems, we assume that for a short period the language under study stays unchanged. The time dimension becomes, through this initial assumption, irrelevant in the description of systems and their structures. But this does not mean that the concept of change does not have any correlate in synchronic linguistics. No change, structural or other, takes place all of a sudden, at the same moment for all speakers. The evolution of the vowel cluster /oi/ in French to /wa/ or /ɛ/ goes on for centuries, and the same is true for the substitution of /j/ for the palatalized *l*. Before these changes and after them we observe two different systems, but it would be absurd to say that we have none during the long span of time when these processes go on. At any moment, between the initiation and the conclusion of these changes, we have a state characterized by the presence of more or less free variants, so that the speakers have the choice between alternative expressions. In each case the choice will be determined by an interplay of factors, some linguistic, some esthetic and social, an interplay so complex that most often the choice will appear as being due to pure chance. At any moment of this long period the French phonemic system would have to be described in such a way that the relations between these variants would be accounted for, because, in my opinion, these relations, however complex they may be, are themselves part of the system. What therefore in the history of a linguistic system appears as a change will in a synchronic description appear as a more or less free variation between different forms of expression, equally admissible within the system.[3] Let it be added that this statement cannot be reversed: every free variation within the system will not, in the time dimension, correspond to a change. It is important to stress this aspect of systems because without admitting a fair latitude of variation within a system, it is difficult to see how structures could change at all. For the study of linguistic interference phenomena affecting the system, this aspect is particularly important, as we shall try to show below.

The other remark I should like to make concerns the thesis that linguistic structures can be described in terms of distinct units, phonemic, morphemic and semantic, without consideration of their substance, i.e. their specific nature as sounds and meanings. Structural descriptions of this kind take the form of an abstract calculus of combinations of units in which sounds and meanings in themselves are irrelevant. Such descriptions have been given and, no matter whether they are possible in all cases or not, they have often led to interesting

[3] In a paper entitled Some Remarks on Norwegian Phonemics (*Norsk Tidsskrift for Sprogvidenskap*, XI, 1939, p. 136–44) I have tried to apply this notion of relations, reversible and irreversible, to a description of the Norwegian system of alveolar consonants as opposed to certain *r*-clusters.

results, throwing fresh light on the nature of linguistic and, in general, of symbolic structures. But if we consider languages in their historical development, substance must be taken into account because of its importance as a conditioning factor in structural changes. In some languages clusters of *r* followed by dental stops or sibilants develop into unique alveolar or cacuminal phonemes—a development obviously conditioned by an apical articulation of the *r*. In the description of the system the articulatory and acoustic features of *r* may be irrelevant, since *r* may be sufficiently defined by its distribution. The substitution of uvular *r* for apical *r* may, therefore, at a given moment be of no structural importance, but it may eventually become important as a feature favoring or checking possible future developments.[4] The same argument probably holds for the semantic substance as well. The introduction into the Western world of Christianity, with its new scales of values, may not, in imperial Latin, have changed the structure of the semantic oppositions around such words as *pater* and *mater*, *bonus* and *malus*, but the new content given to these words may well have been the necessary condition for later rearrangements of semantic patterns.

When languages are viewed as systems or codes, it becomes of primary interest to investigate what happens when linguistic systems come into contact with each other. The way bilingualism affects linguistic systems can be expected to throw light on the basic concepts we use in dealing with isolated systems. The growing interest in bilingualism and in the linguistic interference phenomena caused by it seems to be another aspect of the new approach to the study of language. A recent publication on the subject, to which I shall have occasion to refer several times below, shows this very clearly.[5] Its bibliography contains more than 650 titles of books and papers, and of these more than 90 per cent refer to contributions between 1920 and our own day—precisely the years when structural points of view forced their way not only into linguistics, but into all other social sciences as well.

Bilingualism is a universal phenomenon, since no languages we know have been spoken over long periods of time in complete isolation. It is even possible that bilingualism is one of the major factors in linguistic changes—a point of view which could be defended by good arguments. Code-switching in itself is perhaps not a linguistic phenomenon, but rather a psychological one, and its causes are obviously extra-linguistic. But bilingualism is of great interest to the linguist because it is the condition of what has been called interference between languages.

[4] Uriel Weinreich, *Languages in Contact*, New York 1953, p. 24: "phone substitution, beginning innocuously, may eventually cause a change in the phonemic system of the language that has been affected. The substitution of a uvular ʀ for an apical *r*, for example, may in some languages interfere with /x/ and /γ/ sounds and cause a displacement of a series of phonemes."

[5] The monograph mentioned in footnote 4. As it is a remarkably complete survey of all aspects of language contacts and interference phenomena, I shall frequently have occasion to refer to it, with the abbreviation W.

When an individual belonging to a linguistic community speaking a language A, in certain situations switches to a language B in his intercourse with B-speaking individuals, he is said to be bilingual. The degree of his proficiency in either language may vary, but in most modern publications on the subject the term bilingualism is taken to cover all degrees from total mastery of two (or more) languages to the most rudimentary knowledge of a language other than one's own, native or primary, language. It can even be doubted whether total 100 percent bilingualism exists; it would mean that a person could use either language in any situation, with the same facility and correctness and with the same proficiency as the native speakers. And if such cases exist, it is hard to see how they could interest the linguist, because interference phenomena would by definition be excluded. In the usual cases code-switching will give rise to interference in both directions, from A to B and from B to A, and in extreme cases erratic code-switching may even lead to cases of real mixed languages. The majority of such interference phenomena are ephemeral and individual, others show greater regularities, being repeated over and over again by many speakers. The mechanisms of the interference appear to be the same in both cases, but the linguist is of course mainly interested in those which are not entirely sporadic and individually conditioned, but which exhibit some systematic regularities.[6] Such interference phenomena, spreading from the speech of bilinguals to the speech of monolinguals, can be expected to tell us something about the linguistic conditions of the interference phenomena, and also about the linguistic systems in contact, their similarities and congruences, and their differences.

The best known type of linguistic interference is represented by the so-called loanwords or lexical borrowings. The fact that isolated free morphemes are taken over with such readiness confirms what has been said above about the unsystematic nature of the greater part of vocabulary. When it is maintained that since a language is an integrated system, any addition to it whatsoever must result in the rearrangement of previously existing patterns,[7] this cannot be meant to be rigorously true for the vocabulary as a whole. There are undoubtedly cases of structural rearrangements, when foreign words have been introduced into the structured parts of vocabulary with important consequences. A clear case of this type has been examined in great detail by Knut Bergsland for Lappish dialects in contact with Norwegian. Two Nordic terms of kinship, *mágr* and *sifjungr*, have for special reasons replaced native words, and because of their different content, they have brought about by a trigger effect a complete rein-

[6] W. p. 11: "In speech, interference is like sand carried by a stream; in language, it is the sedimented sand on the bottom of a lake. The two phases of interference should be distinguished. . . . In language, we find interference phenomena which, having frequently occurred in the speech of bilinguals, have become habitualized and established."

[7] Vogt, quoted by W. p. 1.: "To admit that a given element is simply added to the system which receives it without consequences for this system would ruin the very concept of system." I have obviously above all had the morphological system in mind—the statement as it stands is much too sweeping.

terpretation of the Lappish terminology of kinship, by introducing new classificatory principles in the old system. In this particular case the words can be regarded as having semantic existence only as elements of a system, and it would be hopeless and meaningless to study the semantic evolution of each of them independently of the evolution of the system as a whole.[8] In comparative grammar it is common knowledge that there are words which are seldom borrowed—the reasons given being either of a statistical or a psychological nature.[9] For linguists it is perhaps more natural to look for the reason in the fact that these words very often are integrated in relatively coherent semantic structures. But if the word *television*, with the thing it names, should be introduced in Norwegian (and nothing is likely to prevent it) as *televisjon* or as *fjernsyn* (lit. far-sight), it would be hard to maintain that any part of the structure of the Norwegian vocabulary would be altered.[10] A new foreign word, of a very common type, would simply have been added to the vocabulary, or a new Norwegian compound word would have emerged, and no semantic values would have been affected.

Loanwords are, however, apt to affect the phonemic and morphemic system of the receiving language. Even if the phonemes they are composed of are thoroughly assimilated through identification with native phonemes, the new words sometimes introduce phoneme sequences hitherto unknown. The initial cluster *skl-* was unknown in Danish until it was introduced with learned words such as *sklerose*, and if its assimilation has taken place without any apparent difficulty, the reason can only be that the cluster conformed to already existing patterns of initial clusters of three terms—it filled a gap in the system, to use a convenient metaphor, without changing the Danish phonemic system.[11] In other cases the cluster patterns introduced are incompatible with all existing cluster patterns, as for instance *ps-* in the Norwegian words *psykologi*, *psykiatri*, etc. In colloquial speech it is therefore often pronounced *s-*. When it is retained as *ps-* through the pressure of extra-linguistic forces, these loanwords remain deviating units, tolerated but not integrated. If many words of this type are introduced in a language, the result is a split vocabulary, the members of which will belong to two (or more) different phonemic systems. This conclusion is important, because it means either that the description of a phonemic system does not always have to include everything that occurs in speech, or that a phonemic system can sometimes be most conveniently described as the sum of

[8] Knut Bergsland, Det samiske slektskaps og svogerskapsordsystem [The Lappish system of words for relatives and in-laws], in *Norsk Tidsskrift for Sprogvidenskap*, XIII, 1941, p. 148–98, with a summary in French.

[9] W. p. 57: "Relatively infrequent words of the vocabulary are . . . less stable, more subject to oblivion and replacement."

[10] W. p. 53: "Only the most concrete loanwords, such as designations for newly invented or imported objects, can be thought of as mere additions to the vocabulary. For example, the word /televiʒn/, introduced into Amer. German from English, cannot have had much of an effect on existing German vocabulary."

[11] Eli Fischer-Jørgensen, On the Definition of Phoneme Categories on a Distributional Basis, *Acta Linguistica* (Copenhagen) VII, 1952, 1–2 p. 38.

two or more independent systems.[12] Similarly mass influx of foreign words can affect the morphemic make-up of the vocabulary concerned. In languages where the vocabulary is characterized by a relatively high percentage of analyzable words and a correspondingly low percentage of unanalyzable ones—or of unmotivated ones, as de Saussure would say—foreign words, which are by definition unmotivated in the receiving system, will reinforce the latter group and consequently change the character of the vocabulary as a whole.

In the more highly structured domains of language, phonemics and morphology proper, the distinction between the interference phenomena which do affect the system and those which do not is relatively clear. The study of the different types reveals a remarkable parallelism for the two domains mentioned, as demonstrated by the excellent survey in Weinreich's monograph. In both domains substitution of elements with no structural consequences is a frequent phenomenon. Because such substitutions are usually based upon identification of partially overlapping elements, phonetically or semantically, they are most common between relatively congruent systems, e.g. between dialects of the same language. But they also occur between very different languages. When the Armenian dialect of Tiflis, in Georgia, distinguishes, as do Georgian and the other Caucasian languages, between three series of stops and affricates, voiced, unvoiced aspirated and unvoiced glottalized, we have obviously to do with a simple case of substitution, because most, perhaps all, Armenian dialects distinguish three series, although by other articulatory means. But this substitution has not, as far as I can see, had any structural effect in Armenian; the combination rules of phonemes in Tiflis Armenian have remained very different from those obtaining in Georgian. The case is not the same in Ossete, an Iranian dialect which has the same system of stops and affricates as the neighboring Caucasian languages. Here the three-term pattern seems to have replaced an older two-series pattern. Morpheme subtitutions of a similar nature are not uncommon, especially, as Weinreich points out, for closely related languages, where inflectional morphemes can spread "where two prerequisites are satisfied: congruent grammatical structures and *apriori* similar vocabularies." Weinreich concludes with the important observation that "it is ordinarily the one which uses relatively free and invariant morphemes in its paradigm which serves as the model for imitation."[13] Interferences of the opposite type—from the more integrated pattern to the more explicit one—are practically unknown, and even Weinreich can

[12] W., p. 7, rightly points out that "a structural theory of communication which distinguishes between speech and language . . . necessarily assumes that 'every speech event belongs to a definite language' [quoting Lotz]. Only on this assumption is it possible to conceive of an utterance containing some elements which belong to another language than the rest." It may be argued that the argument is circular: In order to describe one system the linguist will have to single out what in the speech events belong to that one system, and disregard the other elements, but in order to be able to do so, he must know the system. The difficulty is a methodological one, but need not create insuperable obstacles in practice.

[13] W. pp. 32 and 41.

quote very few clear instances. In all these cases the system remains basically the same, no new grammatical categories are created, but already existing ones receive, as it were, a more uniform and unambiguous expression. Interferences of structural import in the morphological domain are conspicuously few in the material brought together by Weinreich, most of them belonging to one simple type: the disappearance of morphological or phonemic distinctions under the influence of a system where these distinctions are unknown.[14] Every linguist will be able to quote instances from his own field of experience.

As to interferences which affect the system in the sense that they lead to the emergence of new grammatical categories, it is true that individual cases of code-switching with such results are not at all rare. Weinreich gives striking instances of this type, which show that there are hardly any limits to the "mixing" of systems that can take place under certain given circumstances. The interferences which have lasting structural effects, and which therefore particularly interest the linguist, are rare in Weinreich's material, probably because he does not aim at solving historical problems. It may be, however, that such interference phenomena are not very frequent in linguistic reality either. Systems do not readily change, and structures are, after all, remarkably stable. A given linguistic system will, at any moment in its history, be capable of a limited number of immediate structural innovations; one can say that the system as it is conditions those systems that can be derived from it. The history of a language could therefore be viewed as a continual succession of selections among a limited number of possibilities, all determined by the preceding stage of the system. The actual choice among them will certainly often depend on extra-linguistic factors. Among these factors the influence of a foreign linguistic system, through bilingual groups, is undoubtedly one of the most important, provided there is a minimum of congruence between the systems in contact. One might think that linguistic interferences affect the system only in so far as the foreign elements correspond to some of these innovation possibilities offered by the receiving system. It is in this sense that I choose to interpret Jakobson's statement about phonemics that "a language accepts foreign structural elements only if they correspond to its *tendencies of development*"[15] (italics ours). If this is so, structural changes can always be described in terms of the system in which they take place, without taking into account the foreign influences which must often for several reasons be assumed. It is e.g. a reasonable hypothesis that on many points the word order in Old French is due to Germanic influences. But it is nevertheless possible to describe the evolution of word order rules from Latin to Old French as a succession of steps, of which each one contains in itself, as one evolutionary

[14] W. p. 42.

[15] From *Sur la théorie des affinités phonologiques des langues*, in International Congress of Linguists, 4th, p. 54, quoted by W., p. 25, who adds: "Since such latent internal tendencies, however, by definition exist even without the intervention of foreign influence, the language contact and the resulting interference could be considered to have, at best, a trigger effect, releasing or accelerating developments which mature independently." Terms such as tendencies and latent tendencies would have to be defined formally when dealing with structures.

possibility, the following step, with no mention of possible Germanic influences. The real weight of Germanic influences cannot, at the present stage of our knowledge, be accurately determined, because we cannot know what would have happened if there had been no contact with Germanic speaking peoples. We should simply have to accept as a fact the convergence of Germanic and French linguistic development. It may be argued that such convergences are due to pure chance, but this explanation is not satisfactory when the convergences are observed to take place over very large areas, comprising languages of the most varying stocks. It seems highly improbable that the convergent developments of Western European languages during the last 1500 years should be purely accidental. They could perhaps be explained as reflecting the fact that the Western communities have all, grossly speaking, developed along similar lines, from agrarian economies to modern industrial capitalism, if and only if linguistic structures could unequivocally be correlated with social structures. Since, however, no such correlations have as yet been ascertained, the convergences must be due to continual linguistic contacts, widespread bilingualism, and diffusion of interference phenomena. It is worth noticing that the convergences we have referred to are not merely negatively characterized by the loss of distinctions, but also positively by the creation of new grammatical categories.

The systematic study on a large scale of linguistic convergence phenomena might conveniently supplement the study of bilingualism and interference phenomena in living speech. It offers the possibilities of a "macroscopic" approach, as opposed to the purely "microscopic" one. In investigations of this kind we would have to make use of maps to illustrate the distribution of structural elements, just as maps have been the major tool in the study of the diffusion of phonetic and lexical items in linguistic geography. It may be objected that structural elements cannot be studied independently of the systems they belong to, and that therefore it is impossible to speak about structural elements common to different systems. The objection would, if carried to its exteme consequences, lead to the conclusion that no comparison between systems is possible. Such a conclusion would, however, run counter to what experience and common sense tell us. In the field of phonemics the distinction between voiced and unvoiced stops, of palatalized and unpalatalized consonants are striking structural features which to a certain extent may be studied and mapped out independently of other features of the system they belong to, and independently of the fact that their functions and distribution may be different in the languages concerned. In the field of morphology, the same can be said for common distinctions such as those between definite and indefinite, animate and inanimate in the noun, between active and passive constructions in the verb, and so on. We know that such distinctions spread over large areas among related and unrelated languages. The study of the diffusion of such structural elements would perhaps show more clearly which elements are likely to diffuse and which are not,[16] and what are

[16] W. p. 23: "In studying the spread of sound changes induced by foreign influence, it would be worthwhile investigating whether the results of the several types of interference are diffused in the same manner or with equal ease. The evidence is very scant. . . ."

the linguistic conditions of the diffusion and what are its possible correlations with extra-linguistic facts. Especially for regions where the history of the languages is little known, such geographical methods applied with care could tell us more of linguistic interest than the extremely hypothetical reconstructions of historical relations in a remote past. The structures of a great number of Indian languages in America are fairly well known in their broad outlines, thanks to the brilliant work of Sapir and Boas and their pupils, and it is a question whether the study of structural convergences and congruences would not pay off better than the often hopeless task of disentangling their genetic relations. One cannot deny that such an approach opens the door to many loose generalizations, but there are problems which can not be tackled without taking certain risks. Two things would be needed, cooperation on a large scale among linguists, to assure a maximum of correct information, and great courage. Cooperation should not be impossible, and courage is a quality that is not lacking among linguists, if we are to believe the well known Spanish philosopher Ortega y Gasset[17] who praises "los lingüistas que acaso son, después de los aviadores, los hombres menos despuestos de asustarse de cosa alguna"!

[17] *La Rebelión de las masas*, ch. V.

LINGUISTIC GEOGRAPHY: ACHIEVEMENTS, METHODS AND ORIENTATIONS

GINO BOTTIGLIONI

The development of science includes both the discovery of new facts and the successive equalization of the methods of interpretation. As the methods increase in accuracy, it becomes possible to study other truths which, when they were first noted, seemed to darken that part of the horizon that had appeared clear to still earlier researchers. We have an unceasing contrast of shades and lights that, however, softens and composes itself harmonically to the eyes of those who observe the succession with serene minds while even the latest acquisitions of science get old in the course of time. The same may be said about linguistic geography, which is already half a century old. Now, with the fervor of the first neophytes calmed down and their comprehensible, but often too easy, enthusiasm moderated, linguistic geography can rightly fill the place it deserves among the methods of linguistic studies.

Linguistic geography owes its origin to the comparative method started by Bopp and his close successors who deduced the kinship of Indo-European languages from the observation of the corresponding linguistic phenomena, building their theory on the basis of those phonetic series that were improperly called laws and, in the wrong estimation of Schleicher and even more of the neogrammarians, were interpreted and applied as laws of the physical world.

But since H. Schuchardt[1] opposed naturalism and the strict neogrammarian uniformity with his spiritualistic interpretation of language, considered in the essence of its historical reality, new methodological needs appeared on the horizon of linguistics. And when Paul Meyer,[2] in conformity with the "Wellentheorie" of Schmidt[3] and in a polemic against Ascoli, demonstrated that the location of different isoglottic lines did not coincide in one linguistic area, it became necessary to represent and study each linguistic fact separately, abstracting oneself from those dialectical limits to which Meyer had denied any real objective existence. Among those who agreed with Meyer were also Antoine Thomas and Gaston Paris. The latter, in *Les parlers de France*,[5] showed the way later followed by his pupil Jules Gilliéron, the master of linguistic geography, a way which became an organic part of linguistics, improving and renewing the comparative method with a broader and more realistic view of developmental factors. As a matter of fact, after the first discoveries of Gilliéron, the grave deficiencies of traditional etymology became evident, particularly in the field of the Romance languages. It became clear that traditional etymology, confining

[1] *Über die Lautgesetze. Gegen die Junggrammatiker*, Berlin, 1885.

[2] In *Romania*, 4.294–296.

[3] J. Schmidt, *Die Verwandtschaftsverhältnisse der indogermanischen Sprachen*, Weimar, 1872.

[4] See *Archivio Glottologico Italiano*, 2.385ff. (1876).

[5] Reprinted in *Mélanges linguistiques*, publiés par Mario Roques, Paris, 1909, 432ff.

itself to the relation between the starting-point of an evolution and its conclusion, ignored all intermediate phases, sometimes distorting the history of the word. With perhaps an excess of assurance and absolutism, Gilliéron proclaimed "the failure of phonetic etymology."

The new views of that multiform prism which is human language, illustrated by Gilliéron and his school, evoked a general echo of consent. When in 1919 his lecture on *La faillite*[6] appeared, the *Atlas linguistique de la France* (*ALF*)[7] had already been published, a "monumentum aere perennius"[8] which, as the first geographic representation of linguistic facts seen in a moment of their development, offered to historical-reconstructive research an effective means for the investigation of the complex reasons of their divergence. The principle and the procedure were obvious: just as the geologist moves from the morphological aspect of the ground to discover the sedimentary processes that have determined it, so the linguist needs a faithful representation of the linguistic area to reconstruct its history. Dictionaries were not and are not enough for this purpose. Though no atlas contains as much material as a dictionary, it can, with a limited number of grammatical and lexicological examples, offer a representation, even if synthetic and only sketchy, of a linguistic zone characterized in time and space. In addition, a careful observation and an ingenious comparison of the maps of the *ALF* revealed a series of developmental facts unnoticed by former linguists, and the processes through which innovation takes place appeared in all their complexity.

When Gilliéron undertook his task he may not have realized the consequences that would come from the study of the materials that Edmont, his wise and tireless cooperator, submitted to him, and that he was sketching in on outline maps of France. However, these consequences became evident soon afterwards, upon the completion of the great work, in the studies of Gilliéron himself and of his most faithful disciples, Jean Mongines and Mario Roques.[9] These studies devoted particular attention to phenomena of a psychological nature which had not been considered significant before and stressed expressive peculiarities peculiar to the different social classes, the sexes, and the age groups of the speakers which had been submerged in the levelled neogrammarian studies.

Ethnic reactions and substratum phenomena brought out in Ascoli's learned work were subjected to stricter discipline through areal comparison. Thus for instance the occurrence of cacuminal sounds (such as -*ḍ*(*ḍ*)- < -LL-) in the amphizone of the Mediterranean and farther up to the Indian peninsula actually

[6] Jules Gilliéron, *Étude sur la défectivité des verbes. La faillite de l'étymologie phonétique;* résumé de conférences faites à l'École pratique des Hautes Études; Neuveville, Beerstecher, 1919.

[7] Paris, Champion, 1902–1912; *ALF Corse*, 1914.

[8] This was defined by Meyer-Lübke himself (in *Litteraturblatt für germanische und romanische Philologie*, June, 1902), who in his sincere enthusiasm did not foresee the uncompromising struggle Gilliéron would wage against the neogrammarian school.

[9] Among the many studies of linguistic analysis and reconstruction, we should mention here Gilliéron's *Généalogie des mots qui désignent l'abeille d'après l'Atlas linguistique de la France*, Paris, Champion, 1918.

helps delimit the area of the peoples that were overpowered by Indo-European waves.[10]

The genesis and expansion of innovating currents crossing, interweaving, being superposed on one another, became evident. In an area of mixed languages like Corsica, areal comparison leads us to recognize the most ancient ethnic linguistic aspects (common to Sardinia and Sicily, too) which are partly obscured by the Tuscan stratum;[11] and we can follow the succeeding and more recent French penetration from the principal centers (Bastia, Aiaccio).[12] And if the Corsican sonorization of the voiceless intervocalic occlusives, originally intact, is to be attributed to Tuscan influence, as I think I have demonstrated, the *vexata quaestio* about the priority of Tuscan series like *ago* < ACU over the series *fuoco* < FOCU[13] is explained and settled.

The processes of Latin linguistic expansion could be traced in a new light through the successive strata of Romanized territory. In Sardinia we find derivations of Lat. JANUA, FORNU, ĪLEX and also of Lat. JENUA, FŬRNU and ĒLEX; the presence of the former in the central zone, the most conservative in the island, and of the latter in the peripheral zones, reveals two chronologically different moments of Sardinian Latinization.[14]

The phonetic decay that may bring about the destruction of the form of a word or create homonymies that are intolerable for the linguistic feeling of the speakers, now was ranked among the most important developmental factors in lexical history. A language strengthens weakened words, abandons and replaces embarrassing homophones.[15] The reason why the Romance languages retain CAMBA and not CRUS is to be sought in the weak resistance of the Latin word. In the Gallo-Roman territory especially, where evolution is quicker and more destructive, many Latin monosyllables, dissyllables and trisyllables show a tendency to disappear if they are not strengthened by a suffix, so that SOLICULU > *soleil*, ACUCULA > *aiguille*, CORBICULA > *corbeille*, AETATICU > *âge* replace the doomed words SOLE, ACU, CORBE, AETATE. Against the few, marginal and weak survivals of APIS > *ef*, *es*, we have the flourishing of substitute designations which Gilliéron studied in their genetic relations with so much—perhaps too much—ingenuity.[16] Prov. *clavel* < CLAVELLUS replaces *claus* < CLAVUS which

[10] Bottiglioni, Indice fonetico per l'area di espansione ligure (Abstract from *Atti del I° Congresso Internazionale di Studi Liguri*, 1950); L. Heilmann, Il problema delle cerebrali indiane, in *Scritti in onore di Alfredo Trombetti*, 287–304.

[11] Bottiglioni, La penetrazione toscana e le regioni di Pomonte nei parlari di Corsica, *L'Italia Dialettale* 2.156–210 and 3.1–69.

[12] Bottiglioni, Elementi costitutivi delle parlate còrse secondo l'Atlante Linguistico Etnografico Italiano della Corsica, *Memorie Accad. Bologna, Classe Sc. morali*, S. IV, V. ii (1939/1941).

[13] Bottiglioni, Il còrso pretoscano nella classificazione delle lingue romanze, *Archivum romanicum* 21.524 (1937).

[14] See M. L. Wagner, La stratificazione del lessico sardo, *Revue de linguistique romane* 4(1928), num. 13–14.

[15] See J. Gilliéron, *Pathologie et thérapeutique verbales*, Paris, Champion, 1918.

[16] *Généalogie*, *cit.*, passim.

clashed with *claus* < CLAVIS;[17] *traire* < TRAHERE replaces *moudre* < MULGĔRE, which merged in the struggle with *moudre* < MOLĔRE;[18] in the Gascon area, the cock becomes a vicar (*bégey* < VICARIUM) or a pheasant (*hazan* < PHASIANUM) as GALLUS and GATTUS in the long run converge into the single ambiguous form *gat*;[19] the success of *viande* < VIVENDA is due to the Parisian pronunciation in which *char* < CARNE, pronounced *cher*, gets confused with *chère* < CARA.[20] The innovating effect of these phonetic and semantic conflicts may take place at any time and place; for instance—leaving the French sphere—we notice that in a part of the Italian territory bounded by Sillaro, Santerno and Senio (Emilia-Romagna) the 'hedge' is referred to by derivations of Lat. SAEPE (> *sęva, sęve, siv, sęv*); in another part, these are replaced by derivatives of a deverbative of CLAUDĔRE (> *ćǫda, kį̣ǫda*). The reason for this substitution becomes evident if we observe that in the same territory, the derivations of Lat. SALVIA become *sęv* in the zone of *ćoda* (*kį̣ǫda*), so that superposing the two areas of *ćǫda* and *sęv* < SALVIA, we get perfect congruence.[21]

Besides the comparison of linguistic areas, we derive from linguistic geography the criteria for understanding not only the lexical evolution due to homonymy, but also to different aspects of monosemy and polysemy. If instead of starting from the *idea* reflected in its various denominations (onomasiology) we start from the *word*, we can construct semantic maps where different meanings of the same word characterize mutually exclusive areas (e.g., in Friuli *linda* < *LIMITA 'eaves' and *linda* 'lodge'), others where the two semantic values are superimposed on one another (e.g., in the north of Italy, *balcone* 'window' and *balcone* 'shutter'), and still others where the process of superposition is not completed but where it can be seen in action, or where semantic areas recede and lose contact with each other. The various reasons and conditions of polysemy may be seen in the comparison of semantic areas: thus the meanings have less stability and are weaker in the periphery of a zone, while in the middle they struggle more vigorously until they reach a definite order; the literary language avoids polysemy as much as possible; polysemy is frequent in dialectal speech especially when two ideas are included in the same sphere of human activity; and so forth.[22]

Areal comparison yields results not only in the field of lexicology, but is useful also in the fields of phonetics, morphology,[23] and syntax. Everywhere there is a

[17] Gilliéron, *L'aire "clavellus" d'après l' "Atlas linguistique de la France"*, Neuveville, 1912.

[18] Gilliéron-Mongin, Études de géographie linguistique III. Traire, mulgere et molere, *Revue de philologie française* 20.90–98 (1906).

[19] Gilliéron-Roques, Études de géographie linguistique XII. Mots en collision. A. Le coq et le chat, *Rev. phil. franç.* 24.278–288 (1910).

[20] Gilliéron, *Pathologie, cit.*, I, 2ff.

[21] See Antonio Quarneti, Un caso di omofonia nei dialetti delle alte valli del Sillaro, Santèrno e Sénio, *Memorie Accad. Bologna*, S. IV, V. iii (1939–1941).

[22] For this study of semantic areas, see K. Jaberg, *Aspects géographiques du langage*. Deuxième conférence (aires sémantiques), Paris, 1936, 43–77.

[23] See Jaberg, *Aspects géographiques, cit.* Troisième conférence (aires morphologiques), 79–106.

ferment of studies aiming toward a biology of language, probing deeply into the analysis of thought and of its expression considered as the creative act of the individual in a socially determined environment.

Linguistic geography draws its origin from Gilléron's *Atlas;* none of the earlier works of a similar nature[24] had a sufficient rigor or breadth of view to launch it. Considering the value of the first results of the new methods, one can easily understand the enthusiasm of the master and his disciples and their belief that they had overthrown the whole edifice built up in a century of work by the pioneers of linguistic science and by the neogrammarians. Gilliéron, an eminently critical and caustic mind, thought that the comparison of geographical areas could in itself resolve the most vexed linguistic problems, and thus worked almost exclusively with the materials of the *Atlas.* Notwithstanding the deficiencies pointed out by many people, he thought the *Atlas* to be essentially reliable and used it to destroy the myth of a phonetic law working, like a force of nature, blindly, rigorously, and inescapably. The Gilliéron critique nearly coincides with that of the linguists following Benedetto Croce's idealism;[25] they, too, in stressing correctly the spiritual value of the creative act of language, opposed the neogrammarian naturalism. But at bottom, Gilliéronians and Crocians followed fundamentally different principles and moved from different points of view. The former started from the system, considered in its arrangement, and from perturbations coming from expressive necessities incident to its equilibrium; the latter began with the word in the immediacy of its poetic creation.[26] In addition, both, though working together to throw down the last bulwarks of neogrammarian resistance and to clear the way of science toward new goals, had a one-sided view of the truth, which still escapes us. As we progress in our studies, we continue to discover the faults of our predecessors.

Gilliéron thought that he could discard the services of the historical-comparative method. But as the errors in some of his most daring constructions came to light,[27] the scope of geographical comparison was reducing itself to its proper limits.[28] For, if geographical comparison can point, for example, to homophony

[24] Like the *Petit atlas phonétique du Valais roman* (sud du Rhône), Paris, Champion, 1880, also by Gilliéron; the *Sprachatlas von Nord- und Mitteldeutschland,* Strassburg, 1881 and the 28 maps added to the *Geographie der Schwäbischen Mundart,* Tübingen, 1895.

[25] B. Croce, *Estetica come scienza dell'espressione e linguistica generale,* Bari, 1922; La filosofia del linguaggio e le sue condizioni presenti in Italia, *La Critica* 39 (1941).

[26] See Karl Vossler, *Positivismus und Idealismus in der Sprachwissenschaft,* Heidelberg, 1904; *Sprache als Schöpfung und Entwickelung,* Heidelberg, 1905.

[27] E.g. in the *Abeille* (*cit.*) where, however, Gilliéron shows all the acuteness of his mind and his exquisite linguistic sensibility. For the criticism on this, see Dauzat, *La géographie linguistique,* Paris, Flammarion, 1922, 46ff.; Georges Millardet, *Linguistique et dialectologie romanes,* Paris, Champion, 1933, 42ff., 379ff. and passim.

[28] Karl Jaberg, *Aspects géographiques du langage,* Paris, Droz, 1936, 42: "La géographie linguistique n'a pas la prétension de créer un corps de doctrines particulières. Elle n'est pas, non plus, comme le feraient croire certaines travaux de nôtre maître Gilliéron, fier de la nouvelle voie qu'il avait ouverte, une sorte de "trobar clus". Elle peut cependant revendiquer quelques mérites. . . ." See also M. Grammont, *Rev. Lang. Rom.,* 63.319(1926):

as the cause of a creative impulse, it can say nothing or very little about the reasons why the innovation becomes established in the mind of the speaker in one way rather than in another.[29]

"Quelle est," asks Millardet,[30] "la valeur primitive de l'image contenue dans *bégey* < VICARIUM 'coq'? Est-ce l'écho d'une satire anticléricale, nous reportant à l'inspiration des fabliaux? Ou bien le 'coq' est-il le vice-roi du poulailler, ou bien autre chose encore? Dans quel milieu social a pu se développer tout d'abord le sens nouveau? Le mot est-il savant ou héréditaire?" We cannot answer all these questions by means of linguistic geography alone. But we must also observe that the conception of the death of words due to phonetic decay was based on that very historical phonetics towards which Gilliéron and his first aides showed so much disdain; we can say that the phonetic law or, better, the phonetic series, which had been driven out through the door came in again through the window. The followers of Gilliéron have been speaking of superseding the traditional method, but the work of those who discovered and studied the phonetic "laws" could not have been in vain if many of these "laws" have withstood the sharpest criticism and some of them have been allowed the reconstruction of forms which were subsequently confirmed by historical documents. The pronominal form EO (< EGO), resulting from Romance comparison (Sard. *eo*, Prov. Port. *eu*, It. *io*, Span. *yo*, etc.) found its confirmation in the manuscripts of the sixth century from which we may presume[31] the existence of that form in a former epoch. From the historical sources we derive the certainty which is missing in the results both of mere phonetic comparison and of areal comparison. Fethishists have made great claims for each of the latter methods, but it would be difficult to say which of them is farther from the truth when each of them sticks to positions which are not sustained by the facts. The numerous aspects of the phenomenon of language need different methods of study which must all contribute their best to its reasonable explanation. The facts which are beyond the scope of both phonetic comparison and geographic comparison are numerous. The latter cannot produce the reason for the semantic evolution of *pagus* 'pale limiting a property' into *pagus* 'village', of *paganus* 'inhabitant of the pagus' into *paganus* 'worshipper of the gods', of *vicus* 'block of houses' into *vicus* 'a street', of *aedes, -is* 'fire-place, temple' into *aedes, -ium* 'house of many stories'. The innovations *casa* and *mansio*, preserved in the Romance languages instead of *domus* and *aedes*, are due to political and social causes and depend on factors of civilization[32] which explain also the vitality of *germanus* and the luck of *fratellus* against

"si le comparatisme historique a trop longtemps régné seul, il serait funeste que le comparatisme géographique régnât seul après l'avoir détrôné. On ne saurait faire œuvre utile et solide qu'en combinant ensemble toutes les informations fournies par l'un et l'autre, et en outre toutes les autres ressources dont on peut disposer."

[29] See Walther v. Wartburg, *Problèmes et méthodes de la linguistique*, translated from German by Pierre Maillard, Paris, 1946, 124ff.

[30] Millardet, *Linguistique et dialectologie, cit.*, 66.

[31] Schuchardt, *Vokalismus des Vulgärlateins*, Leipzig, I, 129.

[32] See Alfred Ernout, *Philologica*, Paris, Klincksieck, 1946, 103–118.

frater; and the reasons for the gradual abandonment of *patruus* and *matertera* and of the prevalence of the pair *avunculus—amita*, of the victory of the Graecism *zio, -a* < THIUS < *theîos* are of affective character.[33] The foreign elements, which often penetrate a language and become an integral part of it, are also discovered and arranged chronologically through phonetic and grammatical comparison[34] and through history, rather than through areal comparison. Moreover, we must not forget that homonyms and homophones are not always intolerable to the linguistic feeling of the speakers, and they can coexist conserving their different meaning, e.g., *riso* from 'ridere' and *riso* 'graminaceae', *rombo* from 'rombare' and *rombo* 'a sea fish', *canto* of the voice and *canto* 'corner', *ratto* 'rape' and *ratto* 'rat', *mondo* 'the earth, the universe' and *mondo* from 'mondare', *fondo* from 'fondare' and *fondo* from 'fondere', *porto* from 'portare' and *porto* 'harbor'; *louer* < LAUDARE and *louer* < LOCARE; similarly, though they are spelled differently, *verre* 'a glass', *vair* 'various', *vert* 'green', *vers* 'towards'; *cou* 'neck' and *coup* 'stroke', etc., etc. It follows that homonymy and homophony considered as developmental factors should be used with great caution and not with the almost absolute faith in them which Gilliéron showed in his resourceful constructions.

On the other hand, the certainty of Gilliéron's reasonings and presumed conclusions is opposed at least by the poverty of the materials offered by each linguistic atlas, the richest of which can capture and represent in outline only the most general character of the linguistic territory, displaying it in broad strokes, while the most significant meanings escape it.[35] To this fundamental defect of all the works we are dealing with we should add the unavoidable mistakes made by the explorer of a zone, mistakes that are due, among others, to the way in which the plan of a linguistic atlas is organized and carried out. In so far as the *ALF* itself is concerned, the checks made in the course of time have subsequently revealed its grave deficiencies;[36] and since the atlas is the base on which the promoters of linguistic geography must build, we understand why in the works inspired afterwards by their masters, they have tried to re-examine the methodological standards in the light of the facts produced.

[33] See Paul Aebischer, *Annali della R. Scuola Normale Superiore di Pisa* 5.1–52 (1936); Vittorio Bertoldi, *Linguistica storica* (*Questioni di metodo*), second edition, Roma, Soc. Dante Alighieri, s.a., 65–71.

[34] I cite, from among many, the now classic work of Alfred Ernout, *Les éléments dialectaux du vocabulaire latin*, Paris, 1909.

[35] Wartburg, *Problèmes*, *cit.*, 133: "L'image qu'il donne du trésor linguistique ressemble à un paysage de collines dans une mer de nuages: seuls les sommets émergent; quant aux dépressions surles quelles s'élèvent ces hauteurs et forment le lien organique entre elles, elles restent dissimulées sous le voile opaque des nuages."

[36] See e.g. Dauzat, *cit.*, 11ff., 127ff.; Louis Remacle, Présentation de l'Atlas linguistique de la Wallonie (the first volume of which was published 1953) in *Essais de philologie moderne* (1951); Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liége, fasc. CXXIX, Paris, 1953, p. 245. For the maps of Corsica, P. E. Guarnerio, *Rendiconti Istituto Lombardo* 48.517ff.; C. Salvioni, *ibid.*, 49.705ff.; V. Bertoldi, Vocabolari e atlanti dialettali, *Riv. della Soc. Filol. friul. G. I. Ascoli*, Anno V, fasc. II; Bottiglioni, La penetrazione toscana e le regioni di Pomonte, *cit.*, passim.

The importance of Gilliéron's teaching and the success of the *ALF* appear also from the considerable number of linguistic atlases that were published in Europe and elsewhere from 1910 to 1950 or are still in the process of study or completion.[37] They are all interesting, and some are indicative of new orientations and recent advances in the geographic sector of linguistic study.

It would be useful to deal here in particular with the later atlases, keeping an eye on the methodological criteria followed by Gilliéron, and comparing them with those which have been adopted since. Such criteria concern chiefly: (1) the choice of the points of inquiry; (2) the compilation of the work sheets; (3) the character of the field worker; (4) the character of informants; (5) the method of questioning.

(1) As is known, Gilliéron chose the points of inquiry of the *ALF* quite mechanically, in a geometrical pattern, as it were. Consequently, Edmont was compelled to modify the plan as he went along.[38] This is obviously prejudicial to the objectivity which is required of a field worker, for it involves him in the analysis of the materials he is collecting and a comparison of the different zones—jobs which are the duty rather of the future user of the atlas. That is why I have long maintained[39] that the network of points must be made in advance, chiefly on ethno-linguistic principles derived from previous reliable knowledge of the area under study. This is now done in practice by most people, even by those who do not acknowledge it formally.

(2) The drawing up of work sheets, or questionnaires, has also proceeded quite differently since Gilliéron. He had said that "le questionnaire . . . pour être sensiblement meilleur, aurait dû être fait après l'enquête";[40] hence the best work sheets are those which fit the specific character of the area under study and which result from previous studies of that very area. This is what most people have been doing since the *ALF*.

(3) Another of Gilliéron's standards that we may now consider quite surpassed concerns the field worker. According to the Master, he should not be a linguist in order that a maximum of objectivity may prevail and lest he criticize while he

[37] They are listed, with satisfactory descriptions of their structure, but with judgments of methodological order which are not always reliable, in the weighty and highly meritorious work of Sever Pop (*La Dialectologie. Aperçu historique et méthodes d'enquêtes linguistiques*. Parties I (*Dialectologie romane*), II (*Dialectologie non romane*), i–xxi, 1–1334, Louvain, 1950), as supplemented by Albert Dauzat's remarks and additions, *Le français moderne*, 19e année, num. 3, 1951, 225ff. See also my own remarks in *Rendiconti dell'Istituto di Glottologia dell'Università di Bologna* (IV, 1952), Bologna, S.T.E.B., 1953, pp. 3–4, and in Questioni di metodo nella preparazione degli atlanti linguistici, *Cultura Neolatina* 12.144ff. (1952). For other interesting reports of the latest plans of linguistic Atlases see *Orbis. Bulletin intern. de documentation ling.* 1.87ff. (1952); 2.49ff. (1953).

[38] "De fait il n'est guère de département où nous ayons maintenu le nombre de points d'enquête qui avait été fixé primitivement" (*Atlas linguistique de la France, Notice*, Paris, 1902, p. 4).

[39] In Le inchieste dialettali e gli Atlanti Linguistici, *Atti della XX riunione (settembre, 1931) della Società per il progresso delle Scienze (SIPS)*, Milano, 1932, Vol. I, 413–492.

[40] *Etudes de géographie linguistique. I. Pathologie et thérapeutique verbales*, Neuveville, 1915, p. 45.

collects.[41] Now it is a fact that the field workers of all linguistic atlases since the *ALF* have almost all been linguists: Oscar Bloch, Griera, Gauchat, Tappolet, Scheuermeier, Rohlfs, Wagner, Pop, Kurath and his assistants, etc. etc. This is because we now realize that the autosuggestion and the prejudice feared by Gilliéron cannot influence the man who, linguist or not, is devoted with all his mind to the difficult task of selecting an informant, establishing rapport, and transcribing so smoothly and so quickly as not to interrupt the contact which has been forming and binds the questioner to the questioned. The field worker, even if he is a specialist in linguistics, is so absorbed with this exhausting work that he could not, though he would, think of rules, phonetic laws, linguistic schemes.

The advantages that a native field worker may have over a foreigner in the area under study have also been discussed; but since it is now agreed that the area should be well known by those who work in it, it is plain that a native collector, with training and other necessary qualifications, will always produce better results. Hence a large territory which is linguistically diverse, like Italy or France, should be explored by a staff of field workers, each of them able to get deeply into the specifics of the area in his charge. Gilliéron was not of this opinion, and his skepticism is shared by those who think that it is impossible to get among the various field workers that uniformity of procedure which is necessary to get methodologically uniform materials. I will not try to resume here my discussion of this problem of over twenty years ago.[42] I think that my arguments have since been proved correct by my colleagues, Hans Kurath and his nine able assistants who together compiled the New England Atlas,[43] and by Tomàs Navarro Tomàs who entrusted to eight explorers the collection of the materials for the Atlas of the Iberian Peninsula (*ALPI*), which is to be published soon.[44]

(4) Concerning the criteria of a good informant (sex, age, familiarity with surroundings, amount of education, etc.), we can see both extremes. On the one hand, the authors of the Italian-Swiss Atlas (*AIS*),[45] which marked a considerable advance in metholology, think that "the field worker's rule must be not to stick to any rule."[46] Scheuermeier, one of the able field workers, confirms this: "il n'y a pas de règle infallible pour le choix d'un bon sujet."[47] On the other hand,

[41] *Abeille, cit.*, p. 3.

[42] In *Atti della XX riunione della SIPS, cit.* (footnote 39).

[43] *Linguistic Atlas of New England*, Providence, Rhode Island, Brown University, 1939–1943.

[44] *Atlas lingüístico de la Península Ibérica;* see M. Sanchiz Guarner, *La cartografia lingüística en la actualidad y el Atlas de la Península Ibérica*, Instituto Miguel de Cervantes, Palma de Mallorca, 1953.

[45] K. Jaberg und J. Jud, *Sprach- und Sachatlas Italiens und der Südschweiz*, Bände I–VIII, Zofingen, 1928–1940. Among the great merits of this atlas is the addition of the study and comparison of ethnographic data to those of linguistic facts.

[46] Jaberg-Jud, *Der Sprachatlas als Forschungsinstrument*, Halle (Saale), 1928, p. 191.

[47] P. Scheuermeier, Observations et expériences personnelles faites au cours de mon enquête pour l'Atlas linguistique et ethnographique de l'Italie et de la Suisse méridionale, *Bulletin de la Société de linguistique de Paris*, 23.104 (1932).

Pop requires a rigorous method[48] and enumerates as many as sixteen criteria[49] followed by him in the choice of his subjects for the Rumanian Atlas (*ALR*).[50] No one else, as far as I know, before or after Pop, has felt it necessary to stick to these or similar rules. We may conclude, I think, that except for certain irreducible requirements (certain knowledge of local speech, good pronunciation, intelligence, etc.), the choice of a good informant is entrusted to the intuition and experience of the field worker.

What remains to be ascertained is whether a single informant is superior to a group of two or more. Most linguistic atlases record the answer of one informant; but even Edmont sometimes had to question two or three persons, and the same happened to Griera for his Catalan Atlas (*ALC*),[51] to Scheuermeier and to other field workers for the Italian-Swiss Atlas,[52] and to Pellis for the Italian Linguistic Atlas (*ALI*).[53] Pop distinguishes an inquiry made in a whole country, for which he allows the necessity of several informants, from that made in a given territory, where a single informant is preferable, though the possibility of parallel inquiries remains even here[54]; Bloch, for his Vosges Atlas[55] questioned, besides the principal informant, from two to six secondary ones.[56] For my Atlas of Corsica (*ALEIC*)[57] I used a single informant who was, as it were, responsible, but I admitted the help or corrections of other informants. Thus, even those who accepted the rule of the single informant (authors of the *ALF*, *ALC*, *AIS*, etc.) were often forced to resort to different informants, so that even this principle of the Gilliéron procedure may now be considered superseded once and for all.

(5) Let us deal at somewhat greater length with the use of the work sheets, that is, of the questioning method, for on this point there has been an inclination to carry to the limit a procedure hinted at by Gilliéron, but which is to be attributed particularly to the directors of the *AIS*, followed by Pop and a few others.

I have discussed this subject on several occasions[58] and will briefly recapitulate

[48] Pop, *La Dialectologie*, *cit.*, p. 1156.

[49] *Ibid.*, pp. 723ff.

[50] *Atlasul linguistic român*, sub conducerea lui Sextil Puscariu, part I (by Sever Pop), II (by Emil Petrovici), Cluj, 1938–1940.

[51] A. Griera, *Atlas lingüístic de Catalunya*, Montserrat, 1923–1939. Interrupted at the 5th volume. See Pop, *La Dialectologie*, *cit.*, p. 372.

[52] See Pop, *La Dialectologie*, *cit.*, p. 579.

[53] Promoted by the Società Filologica Friulana G. I. Ascoli, directed by the late Prof. Matteo Bartoli; Ugo Pellis, also deceased, collected almost all the materials now at the University of Turin. See Pop, *La Dialectologie*, *cit.*, 598ff.

[54] *La Dialectologie*, *cit.*, p. 726.

[55] *Atlas linguistique des Vosges méridionales*, Paris, Champion, 1917.

[56] See Pop, *La Dialectologie*, *cit.*, p. 96.

[57] G. Bottiglioni, *Atlante Linguistico Etnografico della Corsica*, Pisa, 1933–1941–1942.

[58] Inchieste dialettali, *cit.;* Questioni di metodo, *cit.;* Il valore unitario e quello obiettivo degli Atlanti Linguistici, *Annali della R. Scuola Normale Superiore di Pisa*, S. II, Vol. i (1932), 167ff.; Come si preparano e come si studiano gli Atlanti linguistici, *ibid.*, S. II, Vol. ii (1933), 126ff.

here. It is a fact that Gilliéron's scheme has no strict rules,[59] so that Edmont, in using, for instance, his herbarium, added certain supplementary explanations which he did not report.[60] We may conclude that neither Gilliéron nor Edmont cared about the strict uniformity of questioning. The same goes for O. Bloch,[61] Le Roux[62] (when he thought he was not well understood, he made an additional interview[63]), and Gardette,[64] whose work sheets were changed several times during the inquiry.[65] On the contrary, the uniformity of questioning is an essential rule of the linguistic collection in the Italian-Swiss Atlas, and it was strictly observed by Pop and Pellis, the late lamented collector of the Italian Linguistic Atlas. This group thought that the answers of different informants are not comparable if they are not elicited in the same way; they lost sight of the true aim of this research, namely, to discover and record for each place the most exact expression of the idea proposed in the work sheets, an expression which will really be useful for comparative purposes.

Is it possible that informants of different character, habits, and culture, when questioned in a uniform way, should reveal that objective truth which we are looking for? A hunter, well versed in the form and life of birds, will react properly when faced with a good picture or drawing, but a worker chosen at random will need detailed, circumstantial oral explanations; moreover, the opportunities which keep arising during the interview for pointing out to the informant an object for whose name we are looking are numberless, and to disregard them seems naïve, to say the least.

The conviction that the best answer is the first, given by the informant as a reaction to the first stimulus of the question itself, goes hand in hand with the belief that the uniformity of the question is essential to the comparative goal. And those who adhere to this method strictly know and admit that the materials they collect do not represent the maximum degree of objective truth, but only the effects that a uniform stimulus produces on different subjects. But this is a subjective truth which does not interest a future user of the atlas; he would like to be able to consider the material offered as a true image of the linguistic conditions in a given territory. For my own part, I have got rid of what I consider as prejudices and, as I have written several times, it is my opinion that the work sheet represents not the *means*, but the *aim* of the inquiry. By a series of words, it sets forth the impressions we want to produce in the informants; the expressions we may want of him may be stimulated and directed by any suitable means and at any occasion that arises during the interview. It follows that those who study

[59] *Notice, cit.*, p. 4.
[60] Pop, *La Dialectologie, cit.*, p. 118.
[61] *Ibid.*, p. 94.
[62] In the *Atlas linguistique de la Basse-Bretagne*, Paris, 1924–1943.
[63] See Pop, *La Dialectologie, cit.*, p. 949.
[64] In the *Atlas linguistique et ethnographique du Lyonnais* (ALL), Institut de Linguistique Romane, Facultés Catholiques de Lyon.
[65] See *Bulletin de l'Institut de Linguistique Romane de Lyon* I, 1953; Pop, *La Dialectologie, cit.*, p. 224.

an atlas compiled by the so-called impressionistic method (uniformity of question, answer at one stroke) may be comparing mistakes, too, while those who use an atlas made without the handicap of any such rules will be comparing the materials that synthesize the real average speech, checked by the one responsible informant. And it will be granted that everybody aims at this, even the adherents of the impressionistic method, so that for instance Sever Pop, who is among the latter, takes care to avoid the danger of "présenter des cartes linguistiques qui ne reflètent pas le parler de la majorité des individus employant encore le patois dans leur entourage."[66]

All agree, then, on the aim: both those whom we should call *rigorists*, that is adherents of a rigorous and mechanical method, like Pop, and the *free collectors*, like the writer himself, who follow a method corresponding to reality and free from the handicap of strict and tyrannic rules. We could continue this discussion, but the remarkable progress of linguistic geography, from the year when the *ALF* appeared to the atlases that now are being planned or completed, can be easily inferred from the preceding discussion. These atlases show that the method of the free collectors is being improved and the number of its followers is growing as the checking of the already published atlases proceeds and experience increases. In fact, the analysis of the deficiencies of the *ALF* has prompted the work of the regional atlases of France, inspired and directed by Albert Dauzat[67] in conjunction, and in full agreement on purpose and method, with the well known Mgr. Gardette, who after the Lyons Atlas is now working on that of the Massif Central. Similarly, work is proceeding on regional atlases in the framework of the impressive Linguistic Atlas of the United States and Canada, as reported by Harold B. Allen in *Orbis* 1.89–94 (1952); the inquiries of R. Hotzenköcherle[68] for the *Linguistic and Ethnographic Atlas of German Switzerland*[69] are

[66] Pop, *La Dialectologie*, *cit.*, p. 1156.

[67] *Nouvel atlas linguistique de la France par régions* (*NALF*). To round out the information in *Le français moderne*, April, 1939, pp. 97–101 and January 1941, pp. 1ff. (see also Pop, *La Dialectologie*, *cit.*, 136ff.), I can add here the latest reports according to a personal communication from Dauzat. The first volume of 220 maps of the Gascon Atlas entrusted to Jean Séguy, Professor at the University of Toulouse, is going to be published; the Atlas Poitu-Charente is almost finished, Miss Massignon having completed the fourth part of the definitive inquiries; M. Loriot has done the field work for the Atlas of the North and Picardy in the whole South of the zone; Dauzat himself is doing with youthful vigor (in spite of his 76 years) the field work for the Arvernian and Limousin Atlas; intensive work is also going on for the Atlas of Provence, entrusted to Ch. Rostaing, who will do the field work at Bouches du Rhônes, his native town; M. Camproux, Luis Michel and Alibert are working on the Atlas of Languedoc; R. Sincou is busy with Quercy, the center of an Atlas Guyenne-Albigeois; the Atlas of the West will be directed by Abbé Guillaume; the Atlas of Normandy has been entrusted to F. Lechanteur who has almost completed his field work on Cotentin and Bocage; Loriot will explore the Seine Inférieure. Further reports will appear in *Le français moderne*. The enthusiasm of my colleague Dauzat is obviously spreading among his able aides and the grand task to which they are devoting themselves will be completed.

[68] See *Bulletin de Lyon*, *cit.*

[69] See the report on it in *Essais de philologie moderne*, *cit.*, pp. 115ff.

also in progress. Spain, too, is working hard: the materials of the *ALPI* should be published soon, while Luis Cortés Vasquez of Salamanca University is at work on the *Atlas lingüistico de Sanabria*—an interesting region in which four linguistic currents join and influence one another. In Italy the publication of the materials collected by the late Ugo Pellis for the *ALI* is now eagerly awaited; while the standards of the field work are not those demanded today, the collection itself is valuable.

There is, then, everywhere intense research and study[70] which shows that linguistic geography has become an essential part of linguistic science. We may say that there is no longer any scholar of historical-reconstructive linguistics who does not feel the need of recourse to areal comparison; such comparison may not be decisive as often as Gilliéron and his first pupils thought, but is always informative on problems not only of modern languages but even of the most ancient ones.[71] It is enough to recall, for the latter, how the theory of the marginal areas started and promoted, after the discovery of Tokharian,[72] a new conception of the original Indo-European guttural in the contrast of the *kentum* and *satem* languages, and illustrated the Basque-Caucasian connections in the frame of Mediterranean languages. Finally, it is likely that further achievements of linguistic geography may contribute enormously to reconcile the Saussurian dualism[73] in the modern orientation of structural linguistics that studies heterogenesis and endogenesis in the perpetual development of different systems. Linguistic geography may yet open to science those new fields outlined more than twenty years ago by N. S. Troubetzkoy when he called for the geographical-phonological description of the languages of the world.[74]

Linguistic Institute of the University
of Bologna

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[70] For the various Atlases which are being issued and drawn or planned, see Pop, *La Dialectologie*, *cit.*, cont. vol. II, pp. 1197–1198.

[71] The set of rules of spatial linguistics Matteo Bartoli dictated in 1925 (*Introduzione alla neolinguistica*, Biblioteca dell'*Archivum Romanicum*, S. II, Vol. xii, Genève) and applied during all his active life devoted to study (see *Saggi di linguistica spaziale*, Torino, 1945) is still very useful in this connection.

[72] See G. Campus, *Due note sulla questione delle velari arioeuropee*, Torino, 1916.

[73] See F. de Saussure, *Cours de linguistique générale*, Paris, 1931.

[74] Cf. *Travaux du Cercle Linguistique de Prague*, 6.228ff. (1931).

IS A STRUCTURAL DIALECTOLOGY POSSIBLE?

URIEL WEINREICH

1. In linguistics today the abyss between structural and dialectological studies appears greater than it ever was. The state of disunity is not repaired if 'phoneme' and 'isogloss' occasionally do turn up in the same piece of research. Students continue to be trained in one domain at the expense of the other. Field work is inspired by one, and only rarely by both, interests. The stauncher adherents of each discipline claim priority for their own method and charge the others with 'impressionism' and 'metaphysics,' as the case may be; the more pliant are prepared to concede that they are simply studying different aspects of the same reality.

This might seem like a welcome truce in an old controversy, but is it an honorable truce? A compromise induced by fatigue cannot in the long run be satisfactory to either party. The controversy could be resolved only if the structuralists as well as the dialectologists found a reasoned place for the other discipline in their theory of language. But for the disciplines to legitimate each other is tantamount to establishing a unified theory of language on which both of them could operate. This has not yet been done.

While the obstacles are formidable, the writer of this paper believes that they are far from insurmountable. The present article is designed to suggest a few of the difficulties which should be ironed out if the theories of two very much disunited varieties of linguistics, structural and dialectological, are to be brought closer together. A certain amount of oversimplification is inevitable, for the 'sides' in the controversy are populous and themselves far from unified. The author would not presume to function as an arbitrator. He simply hopes, without a needless multiplication of terms, to stimulate discussion with others who have also experienced the conflict of interests—within themselves.

If phonological problems dominate in this paper, this is the result of the fact that in the domain of sounds structural and non-structural approaches differ most;[1] semantic study has (so far, at least) not equalled sound study in precision, while in the domain of grammar, specifically structural points of view have had far less to contribute.

2. Regardless of all its heterogeneity, structural linguistics defines a language as an organized system. It was one of the liberating effects of structural linguistics that it made possible the treatment of a language as a unique and closed system whose members are defined by opposition to each other and by their functions with respect to each other, not by anything outside of the system. But since organization must have a finite scope, one of the major problems in a structural linguistic description is the delimitation of its object, the particular system described. Only in ideal cases can the linguist claim to be describing a

[1] Some of the phonological points made here were inspired by N. S. Troubetzkoy's article on linguistic geography, "Phonologie et géographie linguistique," *TCLP* 4.228–34 (1931); reprinted in his *Principes de phonologie*, Paris, ₄1949, pp. 343–50.

whole 'language' in the non-technical sense of the word. In practice he must delimit his object to something less. One of the steps he takes is to classify certain items in his data as intercalations from other systems, i.e. as 'synchronically foreign' elements (e.g. *bon mot* in an otherwise English sentence). Another step is to make certain that only one variety of the aggregate of systems which the layman calls a 'language' is described. These steps are taken in order to insure that the material described is uniform. This seems to be a fundamental requirement of structural description.

To designate the object of the description which is in fact a subdivision of the aggregate of systems which laymen call a single language, the term 'dialect' is often used. But if 'dialect' is defined as the speech of a community, a region, a social class, etc., the concept does not seem to fit into narrowly structural linguistics because it is endowed with spatial or temporal attributes which do not properly belong to a linguistic system as such. 'Dialects' can be adjacent or distant, contemporary or non-contemporary, prestigious or lowly; linguistic systems in a strictly structural view can only be identical or different. It is proposed that the term 'dialect' be held in reserve for the time being and that, for purposes of structural analysis as set forth here, it be replaced by 'variety.'

In deference to the non-structural sense of 'dialect' as a type of speech which may itself be heterogeneous, some linguists have broken down the object of description even further to the 'idiolect' level. This term has been used in the United States to denote 'the total set of speech habits of a single individual at a given time.' The term has been seriously criticized on two grounds: (1) constancy of speech patterns may be more easily stated for two persons in a dialogic situation (a kind of *dialecte à deux*) than for a single individual; (2) there are differences even within an 'idiolect' which require that it be broken down further (e.g. into 'styles').

'Idiolect' is the homogeneous object of description reduced to its logical extreme, and, in a sense, to absurdity. If we agree with de Saussure that the task of general linguistics is to describe all the linguistic systems of the world,[2] and if description could proceed only one idiolect at a time, then the task of structural linguistics would not only be inexhaustible (which might be sad but true), but its results would be trivial and hardly worth the effort.

The restriction of descriptive work to homogeneous material has led to a paradox not quite unlike that proposed by Zeno about motion. A moving arrow is located at some point at every moment of time; at intermediate moments, it is at intermediate positions. Therefore it never moves. Rigidly applied, the typical elements of structural description—'opposition' and 'function of units with respect to other units of the same system'—have come close to incapacitating structural analysis for the consideration of several partly similar varieties at a time. Fortunately, the progress of research no longer requires absolute uniformity as a working hypothesis.[3]

Structural linguistic theory now needs procedures for constructing systems

[2] Ferdinand de Saussure, *Cours de linguistique générale*, Paris, 1949, p. 20.

[3] André Martinet, in preface to Uriel Weinreich, *Languages in Contact*, Linguistic Circle of New York, Publication no. 1, 1953, xii+148 pages, p. vii.

of a higher level out of the discrete and homogeneous systems that are derived from description and that represent each a unique formal organization of the substance of expression and content. Let us dub these constructions 'diasystems,' with the proviso that people allergic to such coinages might safely speak of supersystems or simply of systems of a higher level. A 'diasystem' can be constructed by the linguistic analyst out of any two systems which have partial similarities (it is these similarities which make it something different from the mere sum of two systems). But this does not mean that it is always a scientist's construction only: a 'diasystem' is experienced in a very real way by bilingual (including 'bidialectal') speakers and corresponds to what students of language contact have called 'merged system.'[4] Thus, we might construct a 'diasystem' out of several types of Yiddish in which a variety possessing the opposition /i ~ ɪ/ is itself opposed to another variety with a single /i/ phoneme. Be it noted that a Yiddish speaker in a situation of dialect contact might find information in the confusion of /i/ and /ɪ/ of his interlocutor, which is opposed, on the diasystem level, to his own corresponding distinction. It might tell him (in a 'symptomatic' rather than a 'symbolic' way) where, approximately, his interlocutor is from.

It may be feasible, without defining 'dialect' for the time being, to set up 'dialectological' as the adjective corresponding to 'diasystem,' and to speak of dialectological research as the study of diasystems. Dialectology would be the investigation of problems arising when different systems are treated together because of their partial similarity. A specifically structural dialectology would look for the structural consequences of partial differences within a framework of partial similarity.

It is safe to say that a good deal of dialectology is actually of this type and contains no necessary references to geography, ethnography, political and cultural history, or other extra-structural factors. In Gilliéron's classic studies, the typical (if not exclusive) interest is structural rather than 'external.' In the diasystem 'French,' we may very well contrast the fate of *gallus* in one variety where *-ll-* > *-ɟ-* with its fate in another variety where this phonological change did not take place, without knowing anything about the absolute or even relative geography or chronology of these varieties. Non-geographic, structural dialectology does exist; it is legitimate and even promising. Its special concern is the study of partial similarities and differences between systems and of the structural consequences thereof. The preceding is not to say, of course, that 'external' dialectology has been surpassed; this subject will be referred to below (section 7).

Dialectological studies in the structural sense are, of course, nothing new. Binomial formulas like 'Yiddish *fus/fis* "foot",' which are often condensed to *fis* etc., have always been the mainstay of historical phonology. But it should be noted that structural dialectology need not be restricted to historical problems to the extent to which it has been in the past. Consequences of partial differences between varieties can be synchronic as well as diachronic. The following is an

[4] *Languages in Contact*, pp. 8f.

example of a 'synchronic consequence.' In one variety of Yiddish (we stick to Yiddish examples for the sake of consistency), the singular and plural of 'foot' are distinguished as (*der*) *fus* vs. (*di*) *fis*, while in another variety, both numbers are *fis*. Now, in the number-distinguishing variety, the singular, *fus*, occurs also as a feminine (with *di*); even so, the distinction between singular and plural can still be made in terms of the vowel: *di fus* 'sg.'—*di fis* 'pl.' In the other dialect, *fis* is invariably masculine, perhaps as a consequence of, or at least in relation to, the fact that there only a masculine could distinguish between sg. *der fis* and pl. *di fis*.[5]

If structuralism were carried to its logical extreme, it would not allow for the type of comparisons suggested here: it could only study relations within systems; and since in a perfect system all parts are interrelated ("tout se tient"), it is hard to see how systems could even be conceived of as partially similar or different; one would think that they could only be wholly identical or different. Considerations of this nature prevented orthodox Saussureanism of the Geneva school from undertaking the study of gradually changing systems, since it was felt that languages could only be compared, if at all, at discrete 'stages.'[6] But a more flexible structuralism has overcome this hurdle by abandoning the illusion of a perfect system, and is producing notable results in the diachronic field.[7] We should now take the further step of asserting the possibility of a synchronic or diachronic dialectology based on a combined study of several partially similar systems.

This step in structural linguistic theory would, it seems, do much to bring it closer to dialectology as it is actually carried on.

3. We come next to dialectology's share in the proposed rapprochement. The main objection raised by structuralists against dialectology as usually practiced might be formulated thus: in constructing 'diasystems' it ignores the structures of the constituent varieties. In other words, existing dialectology usually compares elements belonging to different systems without sufficiently stressing their intimate membership in those systems.

In the domain of sounds, this amounts to a non-phonemic approach. A traditional dialectologist will have no scruples about listening to several dialect informants pronounce their equivalents of a certain word and proclaiming that these forms are 'the same' or 'different.' Let us assume four speakers of a language who, when asked for the word for 'man,' utter 1. [man], 2. [man], 3. [mân], and 4. [mân], respectively. On an impressionistic basis, we would adjudge 1 and 2 as 'the same,' 3 and 4 as 'the same,' but 1 and 2 as 'different' from 3 and 4. Yet suppose that informant 1 speaks a variety in which vowel length is significant;

[5] For an example of synchronic consequences in phonemics, see Anton Pfalz, "Zur Phonologie der bairisch-österreichischen Mundart," *Lebendiges Erbe; Festschrift. . .Ernst Reclam*, Leipzig, 1936, pp. 1–19, which is at the same time one of the rare instances of German phonemics and of structural dialectology.

[6] Albert Sechehaye, "Les trois linguistiques saussuriennes," *Vox romanica* 5.1–48 (1940), pp. 30f.; H[enri] Frei, "Lois de passage," *Zeitschrift für romanische Philologie* 64.557–68 (1944).

[7] Cf. the bibliography of diachronic phonemics by Alphonse G. Juilland in *Word* 9.198–208 (1953).

phonemically his form is $_1$/măn/. Informant 2 does not distinguish vowel length, and has given us $_2$/man/. We can further visualize a variety represented by informant 3 where a vowel with maximum degree of opening has the positional variant [å] between /m/ and /n/; phonemically, then, we have $_3$/man/. In the fourth variety, no such positional variation exists; that form is perhaps $_4$/mon/. The structural analysis is thus different from the non-structural one: 2 and 3 now turn out to be possibly 'the same' (but only, of course, if the systems are otherwise also identical), while 1 and 4 appear to be different. Structural linguistics requires that the forms of the constituent systems be understood first and foremost in terms of those systems, since the formal units of two non-identical systems are, strictly speaking, incommensurable.[8]

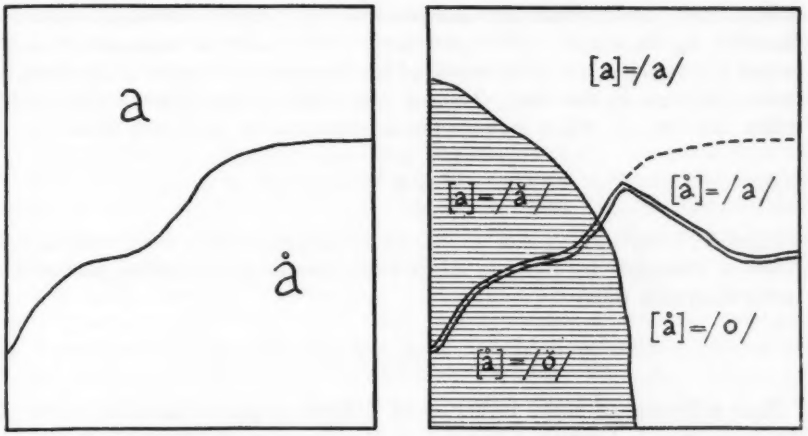
A similar requirement could be made about the units of content, or 'semantemes.' It would not do to say, for instance, that the word *taykh* in one variety of Yiddish is 'the same' as *taykh* in another if, in the one, it is opposed to *ózere* 'lake,' and hence means only 'river,' while in the other it is not so opposed and stands for any sizable 'body of water.' Similar structural cautions would be required with respect to 'synonyms' in the diasystem. In the diasystem 'Yiddish,' *baytn*, *shtékheven*, and *toyshn* all signify approximately 'to exchange,' but they cannot be synonyms on the variety level if they do not all exist in any one variety.

A grammatical example might also be cited. In terms of function within the system, it would not be justified to identify the feminine *vaysl* 'Vistula River' of two Yiddish varieties if in the one it is opposed to a neuter *vaysl* 'eggwhite,' while in the other it is completely homonymous with the (also feminine) word for 'eggwhite.' It is even doubtful whether any two feminines in these two varieties could be legitimately identified in view of the fact that one of the varieties does not possess a neuter gender altogether.

The dialectologist is used to comparing directly the 'substance' of different varieties. The demand of the structural linguist that he consider the train of associations, oppositions, and functions that define linguistic forms seems to the dialectologist complicating, unreasonable, and unnecessary ('metaphysical'). To show up the disagreement most clearly, let us represent the phonic problem just discussed on a map and compare the traditional and the proposed structural treatments of it. Obviously the structural approach involves complications, but the dialectologist will become convinced of their necessity when he realizes that phonemics, like structural linguistics generally, represents not a special technique for studying certain problems, but a basic discovery about the way language functions to which structural linguists are completely committed.

Since, in the structural view, allophonic differences between sounds are in a sense less important than phonemic differences, the 'substantial' isogloss (Map 2) which separates [a] from [å] in the overall /a/ area is structurally somehow less important than the purely formal isogloss which separates pronunciations of [mån] = /man/ from those of [mån] = /mon/; the latter isogloss may not reflect

[8] *Languages in Contact*, pp. 7f.



Map 1: Traditional Map 2: Structural

THE VOWEL IN 'MAN' IN LANGUAGE *X*

On map 2, a continuous single line divides areas with different phonemic inventories (shaded area distinguishing vowel length, unshaded area not distinguishing it). The double line separates areas using different phonemes in this word (difference of distribution). The dotted line separates allophonic differences.

any difference in 'substance' at all; it would not show up on the non-structural map (Map 1). The traditional dialectologist naturally wonders what he stands to gain by the drawing of such 'metaphysical' lines. But if dialectological maps are considered diachronically as snapshots of change, and if it can be shown that the difference between phonemes and allophones can be material in determining sound change, it may be possible to convince the dialectologist that the structural map is after all more true to the reality of functioning language. Similar arguments, perhaps, could also be persuasive insofar as they are pertinent to grammatical and lexical matters.

If dialectologists would consider the functions of the elements which they use in their comparisons, their conception of a 'diasystem' would come close to that proposed here for structural linguistics and might lead to the unified theory which is so badly needed.

4. The partial differences which are proposed as the specific subject matter of dialectologic study may be of two kinds: differences of inventory and differences of distribution. While the latter are the standard material of comparative study, the former have not received their due stress.

As an example of a difference in inventory, let us take a simple phonemic case first. In the following discussion, single slashes enclose sets of phonemes and single tildes designate phonemic oppositions in a variety identified by a preceding subscript number; oppositions in the constructed diasystem are characterized by double tildes, and the formulas for the diasystems are surrounded by double

slashes. Given two varieties with identical five-vowel systems, we might construct the following diasystem: $_{1,2}//\mathrm{i} \approx \mathrm{e} \approx \mathrm{a} \approx \mathrm{o} \approx \mathrm{u}//$. Now let us assume that in one of the varieties, the front vowel of the intermediate degree of openness is more open than in the other; choosing a phonemic transcription which would reflect this fact, we might indicate the difference in the diasystem thus:

$$_{1,2}/\!/\, \mathrm{i} \approx \frac{_1\mathrm{e}}{_2\mathrm{\varepsilon}} \approx \mathrm{a} \approx \mathrm{o} \approx \mathrm{u} \,/\!/ \ .$$

Given two varieties, one of which (1) distinguishes three front vowels, the other (2) distinguishing four, we might formulate the corresponding part of the vowel diasystem thus:

$$_{1,2}/\!/\, \frac{_1/\mathrm{i} \sim \mathrm{e} \sim \mathrm{æ}/}{_2/\mathrm{i} \sim \mathrm{e} \sim \mathrm{\varepsilon} \sim \mathrm{æ}/} \approx \mathrm{a} \approx \mathrm{o} \cdots \,/\!/ \ .$$

Here is the actual vowel inventory of Yiddish considered as a diasystem of three dialects, 1. Central ("Polish"), 2. Southwestern ("Ukrainian"), and 3. Northwestern ("Lithuanian"):

$$_{1,2,3}/\!/\, \frac{_1/\mathrm{i}\text{:} \sim \mathrm{i}/}{\underset{_3\mathrm{i}}{_2/\mathrm{i} \sim \mathrm{ɪ}/}} \approx \mathrm{e} \approx \frac{_1/\mathrm{a}\text{:} \sim \mathrm{a}/}{_{2,3}\mathrm{a}} \approx \mathrm{o} \approx \mathrm{u} \,/\!/ \ .$$

Similarly differences in inventory of grammatical categories might be stated, e.g. between varieties having two against three genders, three as against four conjugational types, and the like. All examples here are tentative and schematic; the possibilities of a more analytical statement of the diasystem, based e.g. on relevant features, remain to be investigated.

One thing is certain: In the study of language contact and interference (see section 5), a clear picture of differences in inventory is a prerequisite.[9]

Differences in distribution cannot be directly inferred from a comparison of the differences in inventory, although the two ordinarily stand in a definite historical relationship. For example, in the diasystem 'Yiddish' described above, the phoneme $_3$/i/ in variety 3 usually corresponds to either $_2$/i/ or $_2$/ɪ/ in cognates of variety 2, and to either $_1$/i:/ or $_1$/i/ in cognates of variety 1 ($_3$/sine/: $_2$/sɪne/: $_1$/sĭne/ 'enmity'). This is, as it were, a correspondence between the nearest equivalents. But many $_3$/o/'s correspond to /u/'s in variety 1 and 2, even though all three varieties today possess both /o/ and /u/ phonemes. Thus, /futer/ means 'father' in varieties 1 and 2, but 'fur' in variety 3; /meluxe/ means $_{1,2}$'craft' and $_3$'state'; /hun/ means $_{1,2}$'rooster' and $_3$'hen.' For the tens of thousands of speakers for whom the contact of these varieties is an everyday experience, these 'Yiddish' sound sequences are not fully identified until the particular variety of Yiddish to which they belong is itself determined. Now no one would deny that a form like Yiddish [fʊl] ($_{1,2}$'full,' $_3$'many') is identified fully only in conjunction with its meaning in one of the varieties, i.e. when account is taken of the differ-

[9] *Ibid.*, pp. 1f.

ences in distribution of sounds in cognates occurring in the several varieties. The less obvious point made here is that the form is not fully identified, either, if relevant differences in *inventory* are not accounted for, i.e. if it is not rendered in terms of the phonemes of one of the concrete varieties: [fil] = $_1$/fɪl/, $_2$/fɪl/, $_3$/fil/.

Recent descriptive work on American English phonemics has come close to treating the language as a 'diasystem' without, however, satisfying the requirements set forth here. The widely adopted analysis of Trager and Smith[10] provides a set of symbols by which forms of all varieties of American English can be described. It makes it possible, for example, to transcribe Southeastern /pæys/ *pass* in terms of some of the same symbols used in /pæt/ *pat* of the same dialect or in /pæs/, /bəyd/ *bird*, etc., of other varieties. This violates the principle advocated here that the phonemic systems of the varieties should be fully established before the diasystem is constructed. We are not told whether in the phoneme inventory of Southeastern American English, the /æy/ of *pass* does or does not correspond as an inventory item to the /æ/ of other varieties. We cannot tell if the [o] of *home* of Coastal New England is the same phoneme, or a different phoneme, from the [ow] in *go* in the same variety. For reasons of this type, the system has been criticized as providing not a phonemic description or a set of descriptions, but a "transcriptional arsenal."[11] Yet the remaining step toward the establishment of a phonemic diasystem is not difficult to visualize.

5. We might now restate and specify the suggested position of structural dialectology in linguistics as a whole. SYNCHRONIC DIALECTOLOGY compares systems that are partially different and analyzes the 'synchronic consequences' of these differences within the similarities. DIACHRONIC DIALECTOLOGY deals (a) with DIVERGENCE, i.e. it studies the growth of partial differences at the expense of similarities and possibly reconstructs earlier stages of greater similarity (traditionally, comparative linguistics); (b) with CONVERGENCE, i.e. it studies partial similarities increasing at the expense of differences (traditionally, substratum and adstratum studies, 'bilingual dialectology,'[12] and the like).

The opposite of dialectology, which hardly needs a special name, is the study of languages as discrete systems, one at a time. It involves straight description of uniform systems, typological comparisons of such systems, and diachronically, the study of change in systems considered one at a time.

6. It was stated previously that diasystems can be constructed *ad hoc* out of any number of varieties for a given analytic purpose. Constructing a diasystem means placing discrete varieties in a kind of continuum determined by their partial similarities. However, in passing from a traditional to a structural dialec-

[10] George L. Trager and Henry Lee Smith, Jr., *An Outline of English Structure* (=*Studies in Linguistics, Occasional Papers* 3), Norman (Okla.), 1951, esp. pp. 27–9.

[11] Einar Haugen, "Problems of Bilingual Description," *Report of the Fifth Annual Round Table Meeting on Linguistics and Language Teaching* (=[Georgetown University] *Monograph Series on Languages and Linguistics* no. 7), in press.

[12] For an essay in bilingual dialectology, see Uriel Weinreich, "*Sábesdiker losn* in Yiddish: a Problem of Linguistic Affinity," *Word* 8.360–77 (1952).

tology, the more pressing and more troublesome problem is the opposite one, viz. how to break down a continuum into discrete varieties. What criteria should be used for divisions of various kinds? Can non-technical divisions of a 'language' into 'dialects,' 'patois,' and the like be utilized for technical purposes?[13]

Before these questions can be answered, it is necessary to distinguish between standardized and non-standardized language. This set of terms is proposed to avoid the use of the ambiguous word, 'standard,' which among others has to serve for 'socially acceptable,' 'average,' 'typical,' and so on. On the contrary, STANDARDIZATION could easily be used to denote a process of more or less conscious, planned, and centralized regulation of language.[14] Many European languages have had standardized varieties for centuries; a number of formerly 'colonial' tongues are undergoing the process only now. Not all leveling is equivalent to standardization. In the standardization process, there is a division of functions between regulators and followers, a constitution of more or less clear-cut authorities (academies, ministries of education, *Sprachvereine*, etc.) and of channels of control (schools, special publications, etc.). For example, some dialectal leveling and a good deal of Anglicization has taken place in the immigrant languages of the United States, and we might say that a word like *plenty* has become a part of the American Norwegian koinê.[15] But in the sense proposed here, there is no 'standardized' American Norwegian which is different from Old-World Norwegian, and from the point of view of the standardized language, *plenty* is nothing but a regional slang term.

Now it is part of the process of standardization itself to affirm the identity of a language, to set it off discretely from other languages and to strive continually for a reduction of differences within it. Informants of standardized languages react in a peculiar way; moreover, it is much easier to deal with samples of a standardized language, to make generalizations about it and to know the limits of their applicability. On the level of non-standardized or FOLK LANGUAGE,[16] a discrete difference between one variety and others is NOT a part of the experience of its speakers, and is much more difficult to supply. For example, it is easy to formulate where standardized Dutch ends and standardized German begins, but it is a completely different matter to utilize for technical purposes the transition between folk Dutch and folk German.

[13] The possibility of introducing some scientific rigor into existing loose terminology has been explored by André Martinet, "Dialect," *Romance Philology* (1953/54), in press. The article by Václav Polák, "Contributions à l'étude de la notion de langue et de dialecte," *Orbis* 3.89–98 (1954), which arrived too late to be utilized here as fully as it deserves, suggests that we call 'language' a diasystem whose partial similarities are grammatical while its partial differences are phonologic and lexical.

[14] Cf. *Languages in Contact*, pp. 99–103. An interesting book about standardization is Heinz Kloss, *Die Entwicklung neuer germanischer Kultursprachen von 1800 bis 1950*, Munich, 1952.

[15] Einar Haugen, *The Norwegian Language in America*, Philadelphia, 1953, p. 588.

[16] Interesting parallels could be developed between the sociolinguistic opposition 'standardized'—'folk' and the social anthropologist's opposition between the cultures of complex (industrialized) and folk societies or strata of society; cf. e.g. George M. Foster, "What Is Folk Culture?" *American Anthropologist* 55.159–73 (1953).

On the whole dialectologists have avoided dealing with standardized languages and have restricted themselves to folk language.[17] Consequently, in practice as well as in theory the problem of dividing and ordering the continuum of language is especially serious with respect to the folk level and not the standardized level. Time was when the continuum of folk language used to be divided on the basis of (usually diachronic) structural features, e.g. the geographic limits of a certain phonological development. Either one isogloss which the linguist considered important was selected (e.g. *k/x* as the line between Low and High German), or a bundle of isoglosses of sufficient thickness was used as a dividing line. In either case, the resulting divisions were not, of course, all of the same degree; they were major, minor, and intermediate, depending on the thickness of the bundle or the relative importance of the key isogloss. It is evident that no unambiguous concept of dialect could emerge even from this optimistic methodology any more than a society can be exhaustively and uniquely divided into 'groups.'

Classificatory procedures of this type are today virtually passé. Dialectologists have generally switched to extra-structural criteria for dividing the folk-language continuum. The concept of language area (*Sprachlandschaft*) has practically replaced that of 'dialect' (*Mundart*) as the central interest in most geographic work,[18] and ever more impressive results are being obtained in correlating the borders, centers, and overall dynamics of language areas with 'culture areas' in a broader sense. Instead of speaking, for instance, of the *helpe/helfe* and *Lucht/Luft* isoglosses as the border between the Ripuarian and Moselle-Franconian 'dialects' of the German Rhineland, linguistic geographers now speak of the Eifel Barrier between the Cologne and Trier areas. This Eifel mountain range happens to be the locus not only of those two random isoglosses, but, among others, also the dividing line between *kend* and *keyk* 'child,' *haus* and *hus* 'house,' *grumper* and *erpel* 'potato,' *heis* and *gramm* 'hoarse'; between short-bladed and long-bladed scythes, grey bread in oval loaves and black bread in rectangular loaves, New Year's twists and New Year's pretzels, St. Quirin as the patron saint of cattle and the same as the patron of horses, two different types of ditty addressed to the ladybug, etc.[19] The line is meaningful as a reflex of a medieval boundary which can in turn be accounted for by more permanent climatic, orological, hydrographic, and other geographic factors.[20]

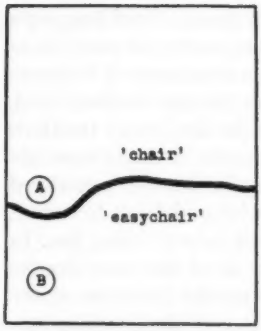
The search for ways to divide the folk-language continuum has also led to

[17] Some people are not averse to calling modern standardized languages 'Indo-European dialects', or speaking of 'literary dialects'. Dialectology in the sense proposed in this paper need not restrict itself to the folk level, but such usage is one more reason why the term 'dialect' ought to be held in abeyance.

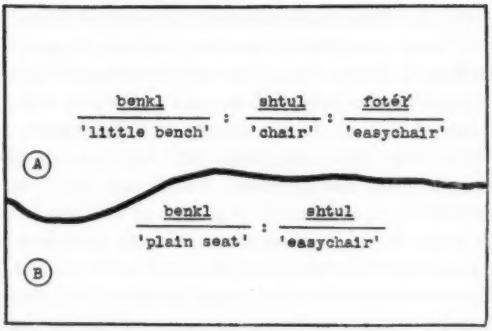
[18] This is particularly evident in the methodologically most advanced German Swiss work; cf. the publications series *Beiträge zur schweizerdeutschen Mundartforschung* edited by Rudolf Hotzenköcherle.

[19] Linguistic data from Adolf Bach, *Deutsche Mundartforschung*, Heidelberg, ²1950, pp. 123ff.; ethnographic data from Adolf Bach, *Deutsche Volkskunde*, Leipzig, 1937, p. 228.

[20] In the United States, Hans Kurath (*A Word Geography of the Eastern United States*, Ann Arbor, 1949), has successfully combined strictly linguistic with 'external' criteria in breaking down the relatively undifferentiated American folk-language area.



Map 3: Meaning of *shtul* in East European Yiddish (Schematized)



Map 4: Designations of Seats in East European Yiddish (Schematized)

statistical correlation methods.[21] Rather than plotting the border lines of single selected structural features, which may be impossible in areas of amorphous transition, the following procedure is used. Inquiries are made at various points concerning the presence or absence of a whole list of test features; then the correlation between the results at some reference point and at all other points is computed, and may be represented cartographically, points with similar correlation coefficients being surrounded by lines which have variously been called 'isopleths' or 'isogrades.' Theoretically related to this procedure are the tests of mutual intelligibility between dialects.[22] All these procedures must depend on an arbitrary critical constant (or constants) for the drawing of a dividing line (or lines, of various degrees of importance), but they do yield an insight into the makeup of a continuously varying language area which supplements, if it does not supersede, the results derived by other methods.

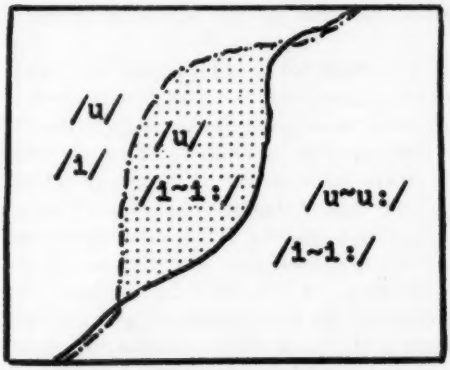
In the domain of dialect sociology, where transitions are perhaps even more continuous and fluid than in dialect geography, the use of extra-linguistic correlations and statistical sampling techniques offers promising possibilities of research in an almost untrodden field.[23]

The use of the social-science tools of 'external dialectology' can do much to supplement the procedures outlined for a structural dialectology. One problem

[21] See David W. Reed and John L. Spicer, "Correlation Methods of Comparing Idiolects in a Transition Area," *Language* 28.348–60 (1952).

[22] Cf. for example C. F. Voegelin and Zellig S. Harris, "Methods for Determining Intelligibility Among Dialects of Natural Languages," *Proceedings of the American Philosophical Society* 95.322–9 (1951).

[23] See the interesting paper by Stanley M. Sapon, "A Methodology for the Study of Socio-Economic Differentials in Linguistic Phenomena," *Studies in Linguistics* 11.57–68 (1953). A scheme for the classification of varieties of a language according to their function (ecclesiastic, poetic, scientific, etc.) to replace the unsatisfactory terminology of 'styles' has been proposed by Yury Šerech, "Toward a Historical Dialectology," *Orbis* 3.43–56 (1954), esp. pp. 47ff.



Map 5: Non-Congruent Vowel-Length Isoglosses in Language *Y*

for combined structural and 'external' linguistic investigation is to determine what structural and non-structural features of language have in fact helped to break up the folk-language continuum into the non-technical units of 'dialects,' 'patois,' etc. This combined research might get to the heart of the question of diasystems as empirical realities rather than as mere constructs. One of its by-products might be the formulation of a technical concept of 'dialect' as a variety or diasystem with certain explicit defining features.

7. Finally a word might be said about the interrelationship of structural and 'external' points of view applied to a specific dialectological problem. Given a map showing an isogloss, the 'external' dialectologist's curiosity is likely to concentrate on the locus of that isogloss. Why is it where it is? What determines the details of its course? What other isoglosses bundle with it? What communication obstacle does it reflect?

The structural dialectologist has another set of questions, stemming from his interest in partial differences within a framework of partial similarity. To take up the semasiological example of Map 3 (which is schematized but based on real data), if *shtul* means 'chair' in zone A, but 'easychair' in zone B, then what is the designation of 'easychair' in A and of 'chair' in B? Every semasiological map, due to the two-faceted nature of linguistic signs, gives rise to as many onomasiological questions as the number of zones it contains, and vice versa. If we were to supply the information that in zone A, 'easychair' is *fotél'*, while in zone B 'chair' is *benkl*, a new set of questions would arise: what, then, does *fotél'* mean in B and *benkl* in A?[24] This implicational chain of questions could be continued further. The resulting answers, when entered on a map, would produce a picture of an isogloss dividing two lexical systems, rather than two isolated

[24] The actual answer is that *fotél'* is not current in zone B, while *benkl* means 'little bench' in zone A.

items (see Map 4). This would be the 'structural corrective' to a traditional dialect map.

It is easy to think of dialectological field problems for the solution of which 'external' and structural considerations must be combined in the most intimate manner. Such problems would arise particularly if the cartographic plotting of systems should produce a set of narrowly diverging isoglosses. Assume that an isogloss is drawn between a variety of a language which distinguishes short /u/ from long /u:/ and another variety which makes no such quantitative distinction. The structuralist's curiosity is immediately aroused about length distinctions in other vowels. Suppose now that the variety that distinguishes the length of /u/ does so also for /i/; but that the isoglosses, plotted independently, are not exactly congruent (Map 5). Some intriguing questions now arise concerning the dynamics of the vowel pattern of the discrepant zone. Nothing but an on-the-spot field study closely combining structural analysis and an examination of the 'external' communication conditions in the area could deal adequately with a problem of this kind.

8. In answer to the question posed in the title of this paper, it is submitted that a structural dialectology is possible. Its results promise to be most fruitful if it is combined with 'external' dialectology without its own conceptual framework being abandoned.

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